

ESSAYS IN PUBLIC ECONOMICS

A Dissertation

Presented to the Faculty of the Graduate School

of Cornell University

in Partial Fulfillment of the Requirements for the Degree of

Doctor of Philosophy

by

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May 2019

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ESSAYS IN PUBLIC ECONOMICS

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Cornell University 2019

My dissertation investigates different aspects of transfer programs in the United States, contributes to our understanding of interactions between safety-net programs, and improves the data available to measure reciprocity of these programs historically.

In my first chapter I investigate how gaining eligibility for Medicaid affects the way families interact with other parts of the safety net and the labor market. The U.S. has many of programs with different eligibility criteria and substantial overlap on a family's budget constraint. I use large expansions of Medicaid eligibility to instrument for the effect of Medicaid on take-up, participation, and eligibility for other programs including cash welfare, food stamps, housing subsidies, and wage subsidies. Combining Current Population Survey data with a difference-in-differences framework, I find that expansions targeting the lowest-income families increased participation in other major programs including food stamps, cash welfare, and rental subsidies. However, effects on program participation obscure larger, offsetting increases in program take-up (participation among eligibles) and reductions in program eligibility. I find that Medicaid expansions increased family labor supply, consistent with the reduction in program eligibility. These spillovers imply that Medicaid expansions for children had two substantial, unintended benefits: more eligible children received safety net benefits and fewer children were eligible due to increased family incomes.

The second chapter, coauthored with Professor Richard V. Burkhauser, in-

investigates how the creation of major safety net programs affected trends in income and its distribution. We extend Census Bureau estimates of the market value of in-kind transfers including Medicare and Medicaid as well as employer-provided health insurance from 1979 back to 1967 using Current Population Survey data and couple it with decennial Census data for 1959. Using these data, we provide a fresh look at the twenty-year period 1959 to 1979 that encompasses the start of New Frontier and Great Society programs. We show that conventional measures of median income and income inequality that exclude the market value of in-kind transfers, including Medicare and Medicaid, will substantially understate the success of government policies in offsetting the stagnation of median market income growth and the rise in market income inequality since 1969.

In the third chapter, I explore the relationship between income and out-of-pocket health care expenditures among the elderly. Health care expenditures have risen rapidly in the United States over the last 50 years, and rising incomes are one potential contributor to this increase. Using an instrument for Social Security income to capture variation exogenously introduced by Congress during the 1970s, I show that less-educated households have a large income-elasticity of demand for health care, as well as additional health insurance coverage and increased health care utilization. The elasticities are sufficiently large to indicate that rising incomes are an important contributor to rising expenditures, at least among the less-educated elderly.

BIOGRAPHICAL SKETCH

James Elwell received a dual B.A. in economics (with honors) and computer science & mathematics from Lewis & Clark College in 2010. He then worked for two years as a software developer in Portland, Oregon before beginning graduate school at Cornell University in 2012. After taking a one-year leave of absence to serve on the Council of Economic Advisers from 2016-2017, James completed his PhD in economics in 2019.

This document is dedicated to all Cornell graduate students.

ACKNOWLEDGEMENTS

I would like to thank Sean Nicholson, Michael Lovenheim, and Samuel Kleiner for their support during my time at Cornell. In serving on my dissertation committee they provided invaluable support and feedback for both my research and in choosing a career. In addition, Mike, Sam, and their coauthors Daniel Grossman and Sarah Cohodes provided code including Medicaid eligibility rules that was invaluable to my first dissertation chapter. I would also like to thank Richard Burkhauser for whom I worked as a research assistant and later a coauthor.

The numerous graduate students at Cornell University also contributed helpful thoughts and advice for my research. In particular, I would like to thank Michiel Paris, Jason Cook, and Amanda Eng for the assistance with program simulations, and Jorgen Harris, Alexander Willén, Anne Burton and David Wasser for their input. Thanks also to Lin Xu and Miriam Larson-Koester for their help preparing for qualifying exams; I would never have reached the dissertation stage without them. Any remaining errors or omissions are my own.

I wish to thank the entire staff of the Council of Economic Advisers for inspiring me and giving me new research ideas and policy interests, but in particular thanks to Sandy Black, Aaron Sojourner, and Stephen Harrell.

Thanks to Christine, Ryan, Sadie, and Max for their friendship and assistance. I grateful to my parents for their support both during and before graduate school. And finally, thanks to Laura for attending and finishing graduate school with me, her support and inspiration, and sharing this next big step in our careers.

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CHAPTER 1

**THE EFFECTS OF EXPANSIONS OF CHILDREN'S MEDICAID
ELIGIBILITY ON PROGRAM PARTICIPATION AND LABOR SUPPLY**

1.1 Introduction

Reforms to government programs or policies may affect the behavior of families towards other programs. Even if programs are not directly linked, cross-program spillovers might occur indirectly if reforms to one program affect elements of behavior that are pertinent to another. Safety net programs in particular have potential for unintended spillovers. The U.S. has many safety net programs that use similar criteria to determine eligibility but often assign different thresholds to overlapping criteria. These programs are administered by separate agencies that do not coordinate implementation. Moreover, the program benefits themselves create highly non-linear budget constraints for low-income families. The combination of overlapping eligibility criteria, uncoordinated implementation, and non-linear budget constraints creates many potential mechanisms operating between programs. For example, if one program is expanded a newly eligible applicant may gain information about eligibility for other programs through a caseworker. Alternatively, changes to a program's eligibility rules may affect families' labor supply decisions, which may affect eligibility for other programs. Understanding these spillovers is critical for policy makers since unintended consequences of changes to program rules may work against the intended goals.

Spillovers affecting program participation fall broadly into two categories:

mechanisms affecting take-up rates and mechanisms affecting eligibility rates.¹ “Participation rate” refers to the fraction of the overall population participating in a given program, and “take-up rate” measures the fraction of individuals who are eligible for a given program and choose to enroll. Existing research has not determined which factors are most important for program take-up in isolation (Currie, 2006), but even less is known about interactions between participation, eligibility, and take-up across various programs. However, understanding interactions between safety net programs is particularly important because these programs are large, may interact through many channels, and potentially affect developmental outcomes of children. In addition, the interactions between safety net programs are presently salient due to recent policy proposals to reform specific programs. For example, efforts to repeal the Affordable Care Act (ACA) included cuts to Medicaid budgets, while proposed rule changes would include implementing work requirements for Medicaid and other programs. There are several major obstacles to evaluating these spillovers, including a lack of exogenous variation in eligibility, concurrent policy changes to major safety net programs, and a lack of data that allow the separation of specific mechanisms.

In this paper, I study how expansions of Medicaid eligibility for children affect participation and take-up of the four major safety net programs targeting families with children: the Supplemental Nutrition Assistance Program (SNAP, known as food stamps until 2008), cash welfare (Aid for Families with Dependent Children or AFDC until 1996, and Temporary Assistance for Needy Families or TANF thereafter), the Earned Income Tax Credit (EITC), and low-income

¹Note that these effects may work in opposite directions; e.g., a Medicaid expansion could increase take-up but cause an offsetting reduction in eligibility for other safety net programs if Medicaid increases labor supply.

rent subsidies.² Specifically, I estimate the change in the fraction of children participating in or taking-up a safety net program in response to a change in the fraction of children eligible for Medicaid. I estimate this effect by parameterizing large expansions of Medicaid and the creation of the Children’s Health Insurance Program (CHIP) that occurred in the late 1980s and 1990s. To isolate plausibly exogenous variation from these expansions, I construct a simulated instrument for Medicaid eligibility (Currie and Gruber, 1996a,b; Cutler and Gruber, 1996). This instrument captures variation due only to changes in Medicaid eligibility rules at the state or federal level.

Changes to rules for any program may affect participation in other programs, but the effects of changes to Medicaid eligibility are of particular interest for several reasons. First, Medicaid is by far the largest transfer program targeting low-income families in the United States, with spending of \$553 billion in fiscal year 2016 and nearly 74 million enrollees (Rudowitz and Valentine, 2017).³ Second, in contrast to other safety net programs that phase out with family income, Medicaid reciprocity is discrete and creates a large discontinuity in families’ budget constraints. By virtue of being the largest program and creating the largest non-linearity in families’ budget constraints, Medicaid has more potential to cause unintended spillovers than other safety net programs.

Finally, Medicaid has been shown to affect a variety of developmental outcomes of children including health and health care utilization, fertility, ed-

²Program participation or take-up always refers to the participation or take-up outcome of a child’s family in other, non-Medicaid, safety net programs. Medicaid participation and take-up are never outcomes I consider. I briefly discuss the effects of Medicaid expansions on additional programs in Appendix Section A.5.

³For comparison, the next largest safety net program targeting low-income families is SNAP, with expenditures of \$64 billion and 42 million enrollees (USDA, 2018). In 2017, 27 million tax filers claimed \$63 billion from the EITC. The number of filers is not comparable to enrollees, however, because it is the number of tax units receiving the EITC and not the number of individuals.

ucation, and long-term labor market outcomes (Currie and Gruber, 1996a,b; DeLeire, 2018; East et al., 2017; DeLeire et al., 2011; Cohodes et al., 2016; Goodman-Bacon, 2016; Brown et al., 2018). These outcomes have important implications for productivity growth, federal budgets, and a whole range of measures of social well-being. However, the effects found in previous studies may be due in part to spillovers to other safety net programs that are also important for developmental outcomes. Estimates of these interactions are important for policy makers considering changes to Medicaid to anticipate the effects of reforms and to understand which mechanisms contribute to overall outcomes.

To estimate the effects of Medicaid expansions, I construct the simulated instrument for eligibility by children's age, race, state of residence, and year using the March Current Population Survey (CPS) for the years 1980-2010. Similar to Cohodes et al. (2016) I estimate both overall Medicaid eligibility, including state-optional rules, and eligibility under only federal rules that are unlikely to be endogenously related to local state characteristics. I account for potential concurrent policy changes to the other major safety net programs by similarly simulating eligibility for those programs. Controlling for these simulated measures of eligibility ensures that my estimates of the effects of Medicaid expansions are not confounded by contemporaneous changes to eligibility of the outcome programs. I then use these simulated eligibility measures to decompose changes in a program's participation rate into changes in take-up and changes in eligibility. Labor supply is an important mechanism driving changes in eligibility. I therefore also estimate the effect of children's Medicaid eligibility on the labor supply of families.

I find that overall Medicaid expansions did not significantly affect partici-

pation in major safety net programs but that federal Medicaid expansions did. A 10 percentage point expansion in children's eligibility under federal rules increases the participation rate in SNAP, cash welfare, and rental subsidies by 0.3, 0.5, and 0.2 percentage points, or 2.3, 4.6, and 3.0 percent relative to mean participation rates, respectively.⁴ However, the estimated effects on participation rates cover up larger, offsetting effects on program take-up and eligibility. Overall and federal Medicaid expansions both increase take-up of SNAP and cash welfare. For both expansion types, the increase in take-up is offset by a reduction in eligibility. The eligibility reductions are larger for overall expansions leading to no significant effect on program participation, while a smaller effect on participation remains for federal expansions. Overall expansions also reduce eligibility for the EITC.⁵

Of the major potential mechanisms affecting eligibility and take-up, I only observe labor supply directly. I estimate that overall Medicaid expansions significantly increased labor supply when measured as average weekly hours worked by families or by family labor income. A positive effect on labor supply could result from families reducing labor supply to maintain coverage when facing strict eligibility criteria prior to an expansion but increasing labor supply when the strict threshold is relaxed. A 10 percentage point expansion in overall Medicaid eligibility increases weekly hours worked by 0.75 and aver-

⁴Throughout, I interpret estimates in terms of a 10 percentage point expansion of eligibility. This is similar to previous work (Cohodes et al., 2016; Dave et al., 2015), but is also a relevant scale given the magnitudes of actual expansions. The standard deviations for fraction of children eligible under federal and overall Medicaid expansions are 0.08 and 0.13, respectively. From 1980 to 2010 federal Medicaid eligibility increased by 15.1 percentage points, and overall eligibility by 38.4 percentage points.

⁵I estimate effects on participation, take-up, and eligibility for SNAP and cash welfare. For the EITC, the CPS contains imputed, not actual, receipt. Thus, I can only estimate effects on EITC eligibility, not actual participation or take-up. Conversely, the CPS contains insufficient information to estimate eligibility for rental assistance, and so I am only able to estimate effects on participation for rental subsidies, not effects on eligibility or take-up.

age annual income by more than \$2,000, or 1.3 percent and 3.6 percent relative to means, respectively. Federal Medicaid expansions do not significantly affect the labor supply of families, although the point estimates are consistent with a small increase in family labor supply. Similar to prior work on labor supply, I find no significant effects of Medicaid expansions on the labor supply of single mothers. Instead, labor supply estimates are driven by teens, other adult family members, and families with two parents. The positive effect of Medicaid expansions on labor supply is consistent with the reductions in eligibility for safety net programs.

This paper makes several contributions to the literature on Medicaid eligibility, program participation and take-up, and labor supply. First, I estimate the effects of expansions of Medicaid eligibility on participation in the major safety net programs targeting low-income families with children while controlling for the simulated eligibility for those programs. Prior work has investigated participation in some of these programs, no work has controlled for contemporaneous changes to program rules that are a potential threat to identification (Blank, 1989; Winkler, 1991; Moffitt and Wolfe, 1992; Yelowitz, 1995; Ham and Shore-Sheppard, 2005; Shore-Sheppard, 2008; Decker and Selck, 2012; Corson and McConnell, 1990; McConnell, 1991; Yelowitz, 1996). Second, I decompose the effects of Medicaid expansions on program participation into changes in take-up of other safety net programs and changes in eligibility for those programs. This decomposition is important because I show modest changes in program participation mask larger but offsetting effects of Medicaid expansions on take-up and eligibility.

Third, I estimate the effects of Medicaid expansions for children on family

labor supply, accounting for spillovers within families. There is a large prior literature on the effects of Medicaid expansions on labor supply, but this research only considers the labor supply of single mothers (Winkler, 1991; Moffitt and Wolfe, 1992; Yelowitz, 1995; Montgomery and Navin, 2000; Meyer and Rosenbaum, 2001; Ham and Shore-Sheppard, 2005; Pohl, 2018). However, other adults in single mother families, such as older siblings, and the members of two-parent families may have more elastic labor supply if single mothers provide their own child care. Prior research has found that single women have relatively inelastic labor supply (McClelland and Mok, 2012). I provide evidence that other adults and teens do adjust their labor supply more flexibly. Additionally, by considering families with alternative parental structures, I estimate the effect of Medicaid expansions on a much larger sample of treated families. After the implementation of the CHIP expansions nearly all states covered children in families with incomes below 200 percent of the federal poverty line (FPL). In 2017, there were 17 million children in families with single mothers in the U.S., fewer than 10 million of which lived in poverty (U.S. Census Bureau, 2017, 2018b). However, 28 million children lived in families with incomes below 200 percent of the FPL (Fontenot et al., 2018). Thus, previous research has missed two important margins on which labor supply may be affected by Medicaid expansions.

Finally, I build on previous work by explicitly testing the exogeneity of state-optional Medicaid expansions. State expansions may be related to local demographic trends or economic shocks. Cohodes et al. (2016) evaluate these state-level expansions by developing a measure of Medicaid eligibility that uses only changes to federal Medicaid rules, which are unlikely to be related to the characteristics of specific states. They find similar results from estimates using the overall and federal measures of eligibility, providing informal evidence

supporting the exogeneity of state-level expansions. However, the federal and state-optional expansions also differ by where in the income distribution these expansions are concentrated. Figure 1.1 shows the fraction of children eligible for federal and state-optional expansions by their families' income percentile. Federal expansions tend to cover children in very low-income families, while state-optional expansions cover children in relatively higher income families, a result also shown by Goldsmith-Pinkham et al. (2018). Cohodes et al. (2016) study educational attainment, and it is unsurprising that Medicaid expansions would increase educational attainment for children in both low- and relatively high-income families.

It is theoretically ambiguous whether Medicaid expansions would affect safety net program participation of low- and higher-income families in the same direction, however. For example, higher income families are more likely to have two parents, and thus more flexibility to modify their labor supply. As a result their program participation behavior may be more sensitive to eligibility mechanisms than low-income families. I extend the informal test conducted by Cohodes et al. (2016) and contribute to the literature by estimating an explicit test of the relative exogeneity of the state-optional expansions. I select a subsample of children covered by state-optional expansions whose families' incomes are similar to those covered under federal expansions and show that the effects for these families are broadly similar. The similarity of these results corroborates the exogeneity of state-optional Medicaid expansions with respect to local demographic and economic characteristics.

Overall, my results indicate that Medicaid expansions targeted towards low-income and single-parent families increased program participation, while over-

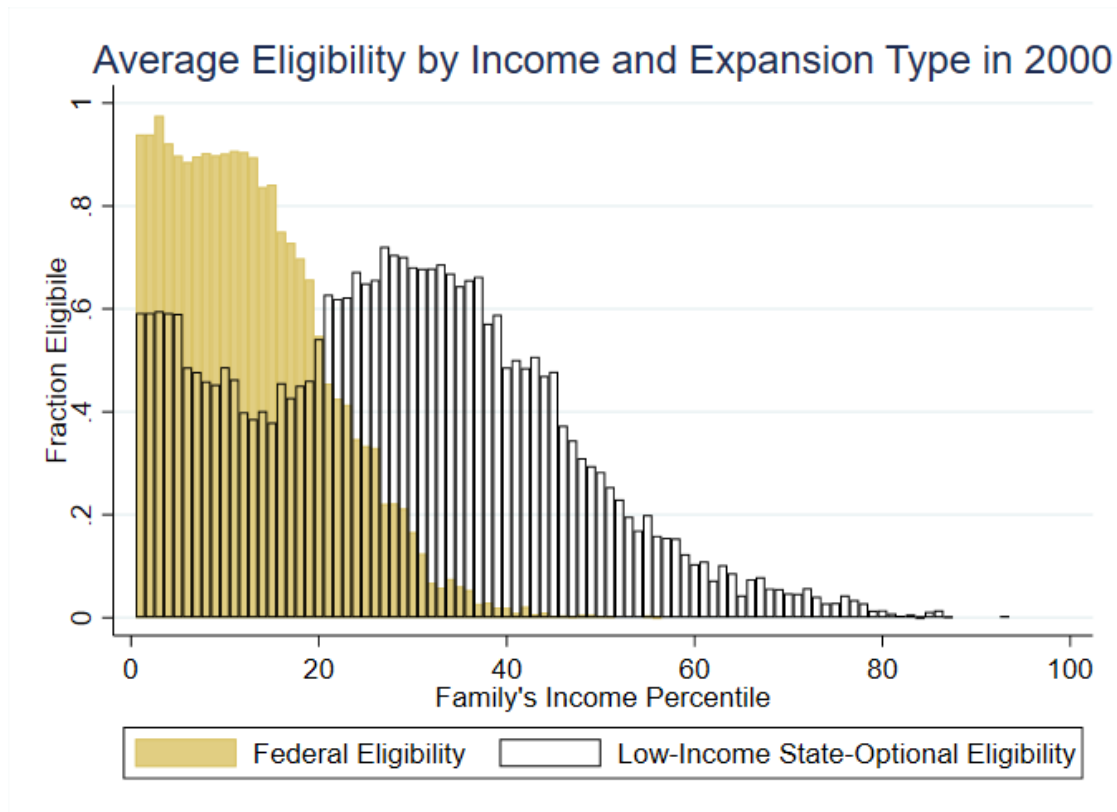


Figure 1.1: The figure shows the fraction of children age 0-17 eligible for federal and state-optional Medicaid expansions by the income percentile of the children's families in the year 2000. Federal eligibility uses only federal Medicaid rules holding AFDC rules fixed to 1980. State-optional expansions include all state expansions to Medicaid and CHIP. Income percentiles are determined using families' gross income.

all expansions did not. This finding is unsurprising as the relatively higher-income families affected by overall and state-optional expansions are less likely to be eligible for the major safety net programs. The modest effects of Medicaid expansions on program participation obscure offsetting, relatively larger effects of expansions on program take-up and eligibility. Medicaid expansions increased program take-up, consistent with an increase in information regarding other safety net programs or a reduction in the transaction costs of applying,

but caused an offsetting reduction in eligibility for other safety net programs, corroborated by an increase in labor supply. These estimates suggest that moving from relatively strict to relatively generous eligibility thresholds for Medicaid did not adversely affect labor supply. Additionally, my results provide evidence of significant spillovers between safety net programs. These results inform the interpretation of prior work on the effects of Medicaid expansions, as estimates of the short- and long-term effects of Medicaid expansions combine the effects of Medicaid eligibility itself with the benefits of increased take-up of other safety net programs.

The remainder of this paper is organized as follows. Section 1.2 describes the institutional background and theoretical intuition of the effects of Medicaid expansions. Section 1.3 provides a brief overview of the most relevant literature. Section 2.3 describes my data and construction of my outcomes and simulated instrument. Section 2.4 presents my empirical model and discusses identification. Section 2.5 presents my primary results, and Sections 1.7 and 1.8 present evidence on instrument exogeneity and robustness checks, respectively. Section 2.6 concludes.

1.2 Medicaid Institutional Background

1.2.1 The Medicaid Program

Medicaid was created in 1965 to provide health insurance to low-income families. Each state implements its own Medicaid program, with some flexibility in determining program parameters, but each state's program must follow ba-

sic coverage rules and minimum eligibility thresholds set by the federal government. States may optionally expand Medicaid programs beyond federal minimum eligibility thresholds, and the federal government provides matching funds for program costs up to a maximum eligibility threshold. The coverage rules set by the federal government have gone through several periods of expansion since the program's inception. These expansions, combined with the joint state-federal program structure, create substantial variation in eligibility within and across states that I use to estimate the effects of Medicaid expansions on program participation.

Low-income families with children originally gained Medicaid eligibility through enrolling in AFDC (cash welfare). AFDC eligibility levels were set by states and were generally well below the federal poverty line. This created substantial cross-state variation in eligibility for Medicaid, as some states set more generous AFDC eligibility levels. Beginning in the mid-1980s, a series of federal laws gradually decoupled Medicaid eligibility for families with children from AFDC by establishing a separate pathway to eligibility. This alternative pathway used substantially higher, and nationally uniform, income eligibility thresholds for children and pregnant women. In addition, these changes relaxed constraints related to the structure of the family and determined eligibility for individuals within the family rather than the family as a whole, as under AFDC.

By 1990, states were required to cover children under age six in families with incomes below 133 percent of the FPL, and older children in families with incomes under 100 percent of the FPL. An important feature of these expansions is that they applied only to children born after September, 1983. This sharp birth-date cutoff led to large within-age increases in eligibility as children aged into

the expansions and a gradual increase in overall coverage throughout the 1990s. Many states optionally extended even more generous coverage. CHIP was created in 1997 to further expand health insurance coverage for children, and by July 2000 all 50 states and Washington D.C. had implemented CHIP programs. While all states created CHIP programs, the timing and eligibility thresholds were largely left to the discretion of the states. CHIP programs expanded eligibility further up the income distribution to families with incomes up to 200 or 300 percent of the FPL, and in many states expanded coverage levels for older children. I provide more detail on specific Medicaid and CHIP expansions in Appendix Section A.1. I use the variation in Medicaid eligibility across states, over time, and within ages induced by these expansions to separately estimate the effects of overall and federal Medicaid eligibility on participation in other (non-Medicaid) safety net programs and labor supply.

1.2.2 Theoretical Motivation

There are several mechanisms through which Medicaid eligibility and program participation may interact, which fall into two categories. The first category is take-up mechanisms. These mechanisms directly affect the decisions of already-eligible families of whether to enroll in other safety net programs. There are three main take-up mechanisms. First, through enrolling in Medicaid families may learn about their eligibility for other programs. For instance, this information may be provided to them by a caseworker when enrolling, and in some cases they may be actively encouraged to apply to other programs.⁶ The information channel should unambiguously increase participation rates in other

⁶This was the case for the Oregon Health Insurance Experiment (OHIE) (Baicker et al., 2014).

programs.

Second, Medicaid eligibility may affect program participation through stigma (Moffitt, 1983). If enrolling in transfer programs is stigmatizing, families may not participate. Newly eligible Medicaid applicants may accept the stigma for Medicaid if it is more valuable or less stigmatizing than other programs, but may then choose to enroll in other safety net programs as well. Alternatively, if a family is already enrolled in other transfer programs but Medicaid is less stigmatizing, then an expansion may allow them to enroll in Medicaid and unenroll from other programs if Medicaid sufficiently relaxes their resource constraints. It is also possible that Medicaid expansions reduce overall stigma for all safety net programs. Thus, the effect of stigma on program participation is ex-ante ambiguous. Third, there are transaction costs for applying to safety net programs. Changing eligibility for Medicaid may affect the transaction costs of applying to other programs. To the extent that applications require similar information Medicaid expansions would increase program take-up, but expansions would decrease take-up to the extent that applications are time consuming and applying for Medicaid crowds out applying to additional programs. Like stigma, the effect of transaction costs on program participation is ambiguous.

The second category of mechanisms through which programs may interact are eligibility mechanisms. These mechanisms indirectly affect program participation rates by changing the eligibility status of families. The primary eligibility mechanism is labor supply.⁷ Medicaid expansions may increase labor supply

⁷Other potential eligibility mechanisms include, for instance, decisions regarding fertility or marital behavior of families, which could theoretically be affected by eligibility for Medicaid and would affect eligibility for other programs. DeLeire et al. (2011) estimate the effects of Medicaid expansions on fertility and do not find robust evidence of any relationship. Substantial work has looked at the relationship between Medicaid or other safety net programs and marriage, and has found at most small effects. See Gruber (2003a).

through improving the health of covered pregnant women or working teens. Additionally, Medicaid expansions may change incentives to work by modifying non-linearities in a family's budget constraint. In a simple static model of labor supply, increasing eligibility thresholds for Medicaid shifts the income-level at which a family experiences a large, discontinuous reduction in income due to the loss of Medicaid eligibility because of Medicaid's discrete eligibility feature, or the "Medicaid Notch." If families modify their labor supply in response to their new budget constraint, their eligibility for other transfer programs may also be affected. Unlike improvements in health, changes in budget constraints faced by families could affect program participation and labor supply in either the positive or negative direction, and in general the direction will depend on a family's income relative to pre- and post-expansion eligibility thresholds. Therefore, the net effect of changes in labor supply also is ex-ante ambiguous.

To clarify the intuition and theoretical predictions of a simple static model, Figure 1.2 shows a simplified budget constraint for a single mother with a single child under age six in 1989, prior to the Omnibus Budget Reconciliation Act (OBRA) of 1989 expansion. The black line (alternating short and long dash) is the labor-leisure trade-off for a family that does not participate in any transfer programs. The blue (short dash) and red (long dash) lines add the AFDC/SNAP benefits and EITC, respectively. The green (solid) line shows the budget constraint including the value of Medicaid coverage for the child. Once the family's AFDC benefit is phased out, the child loses Medicaid eligibility and there is a large, discontinuous drop in family income including Medicaid. Consider a family with potential income equal to point A. With a conventional utility function, this family will choose to reduce its labor supply to gain Medicaid eligibil-

ity. In this example, such a shift in income would make this family also eligible for SNAP and AFDC but would not affect EITC participation. If the mother cannot adjust labor supply continuously but instead must choose to work full-time, part-time, or exit the labor force, the family's reduction in labor supply and resulting change in program participation may be large.

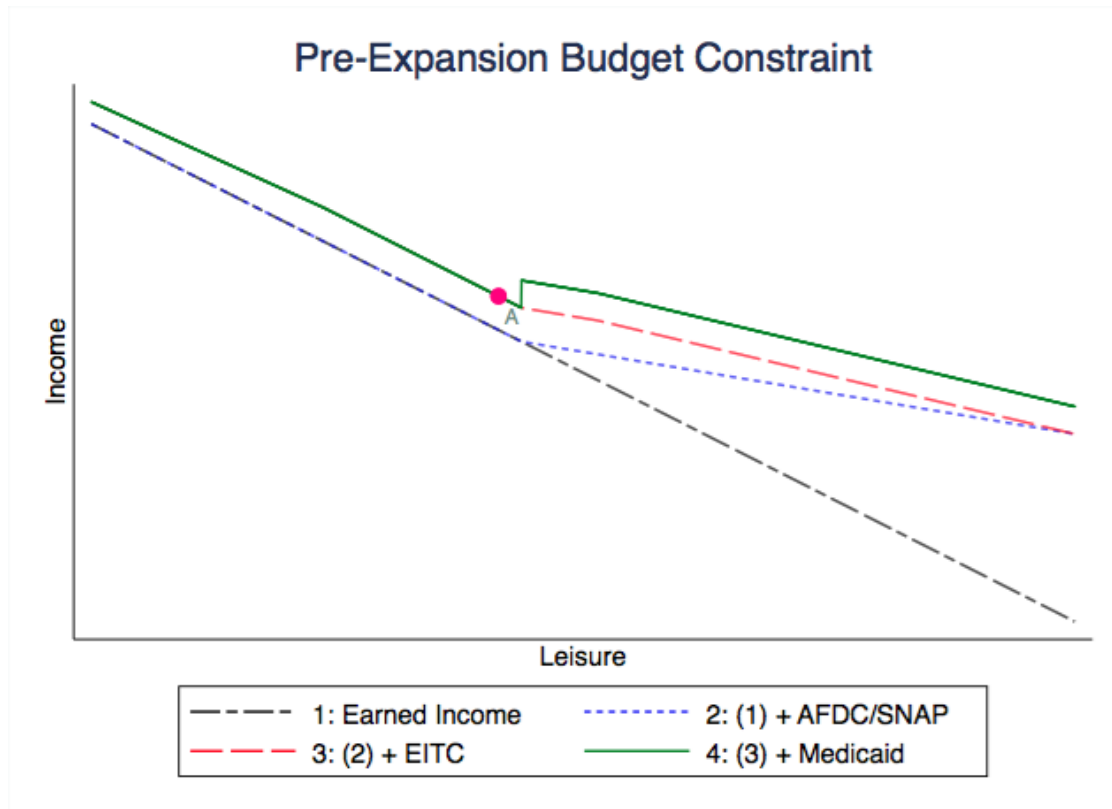


Figure 1.2: Budget constraint for a single mother with one non-disabled child under six in 1989, pre-expansion. Black (alternating short and long dash) lines show earned income. The blue (short dash) and red (long dash) lines add AFDC/SNAP and EITC, respectively. The green (solid) line adds Medicaid income for one eligible child. Mother's own eligibility is not included. Figure is drawn to scale assuming mother earns twice the federal minimum wage of \$3.35 and that the family values the child's Medicaid coverage at its market value.

Figure 1.3 shows the same family's budget constraint after OBRA 1989 went

into effect. States are now required to cover children under age six until family income reaches 133 percent of the FPL. The effect of the expansion on a family's program participation and labor supply depends on their position in the income distribution. Families with potential income equal to point A no longer face an incentive to reduce labor supply to gain Medicaid eligibility. These families may increase their labor supply following the expansion. Families with potential income at point B pre-expansion had incomes sufficiently high that Medicaid did not distort their labor supply decisions. Post-expansion, they now face an analogous situation as families at point A did pre-expansion, and may reduce their labor supply.⁸ In this example, the expansion would reduce AFDC/SNAP receipt for families with incomes near point A but would not affect program participation of families with incomes near point B. The value of Medicaid is shown to be constant, but this is not necessarily the case. Health is inversely correlated with income, and poorer families may place a higher value on Medicaid if they are more likely to use its services. Conversely, higher income families may place a lower value on Medicaid if they are more likely to have alternative options for health insurance, such as private coverage through an employer.

In reality, the family's labor supply will depend on additional factors. This simple model presents only a subset of safety net programs at a single point in time, although rules for these programs also vary over time and across states. There may be heterogeneity in the ability of families to modify their labor supply or their knowledge of program eligibility rules. Families with multiple potential workers may be more responsive to expansions because additional potential earners may have more flexibility to choose their hours, whereas a family

⁸Note that I do not discuss all the possible changes to labor supply implied by even this simple static model, and families located at other points on the income distribution also face changes to their incentives. I focus on the two cases for which the intuition is most important for this context. See Blank (1989) and Bitler et al. (2006) for additional discussion.

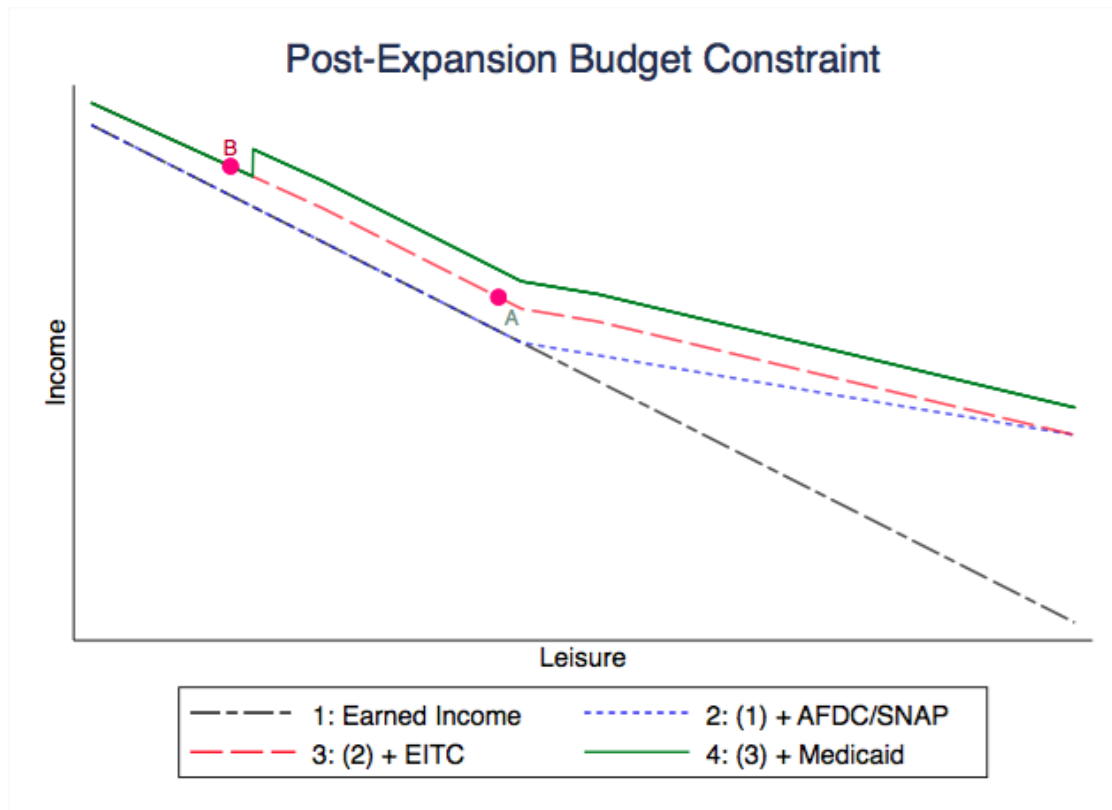


Figure 1.3: Budget constraint for a single mother with one non-disabled child under six in 1989, post-expansion. Black (alternating short and long dash) lines show earned income. The blue (short dash) and red (long dash) lines add AFDC/SNAP and EITC, respectively. The green (solid) line adds Medicaid income for one eligible child. Mother's own eligibility is not included. Figure is drawn to scale assuming mother earns twice the federal minimum wage of \$3.35 and that the family values the child's Medicaid coverage at its market value.

with one earner such as a single mother with children may have less flexibility due to being constrained to provide her own child care. Similarly, two-parent families may have more flexibility to pay the transaction costs of enrolling in multiple programs, but single-parent families may have a better understanding of program eligibility rules as safety net programs in the U.S. have historically explicitly targeted families with children and single mothers.

These different mechanisms may operate simultaneously and in opposing directions. The CPS data do not contain sufficient information to distinguish between the alternative potential mechanisms within the take-up and eligibility categories. As a result of data constraints, most prior literature (discussed below) has estimated the net effect of Medicaid expansions on program participation only, and has not separated the mechanisms driving these effects nor distinguished between the eligibility and take-up mechanisms.⁹ By simulating eligibility for some safety net programs I am able to separate the net effects of the take-up and eligibility mechanisms. However, I cannot disentangle the effects of mechanisms within these categories. Distinguishing between these categories is particularly important if, for example, Medicaid expansions increased take-up of other safety net programs directly through the information mechanism but reduced eligibility for other safety net programs through the labor supply mechanism. In this case, these mechanisms would partially or completely offset, and estimates of the effects of Medicaid expansions on program participation rates may find no overall effect that covers up substantial offsetting take-up and eligibility effects.

1.3 Literature Review

There is a large literature studying the effects of Medicaid coverage on a variety of outcomes including health and health care utilization, inter-generational health, mortality, crowd-out of private health insurance, fertility, bankruptcy, education, and long-term labor market outcomes (Currie and Gruber, 1996a,b,

⁹Yelowitz (1996) is an exception, I discuss his method for separating the labor supply and information mechanisms in the next section.

2001; Boudreaux et al., 2016; DeLeire, 2018; East et al., 2017; Goodman-Bacon, 2018; Cutler and Gruber, 1996; Card and Shore-Sheppard, 2004; Shore-Sheppard, 2008; Gruber and Simon, 2008; DeLeire et al., 2011; Gross and Notowidigdo, 2011; Cohodes et al., 2016; Goodman-Bacon, 2016; Brown et al., 2018). Within the larger literature on Medicaid, I contribute directly to the literature on program participation and take-up, and the literature on labor supply.

However, my results are also relevant to the broader literature. Many outcomes affected by Medicaid eligibility are also affected by other safety net programs. For example, SNAP participation has been found to improve a variety of outcomes including health and educational attainment (Almond et al., 2011; Council of Economic Advisers, 2015; Hoynes et al., 2016). Because Medicaid expansions increase SNAP take-up, the overall effect of Medicaid expansions on outcomes such as health and educational attainment is due to the direct effect of expanded Medicaid coverage and the indirect effect of increased SNAP take-up. The cross-program spillovers do not change the overall effects estimated by prior research, but do indicate that these spillovers are important for researchers and policy makers to understand mechanisms driving overall outcomes.

1.3.1 Medicaid, Program Participation, and Program Take-up

The first literature I contribute to investigates the effects of Medicaid coverage for children on program participation and take-up.¹⁰ A large set of studies

¹⁰A related literature examines expansions of Medicaid eligibility for childless adults, but I consider only expansions affecting children's eligibility. Moreover, expansions for childless adults may have different effects on program participation as safety net programs in the U.S. provide little or no benefits to childless adults. Therefore, I summarize this literature briefly. Baicker et al. (2014) and Agirdas (2016) study state-optional expansions for childless adults and find that these Medicaid expansions increased SNAP participation. Baicker et al. (2014) find

have investigated the effects of Medicaid expansions on program participation in several safety net programs, particularly AFDC. Decker and Selck (2012), Blank (1989), Winkler (1991), Moffitt and Wolfe (1992), Yelowitz (1995), Ham and Shore-Sheppard (2005), and Shore-Sheppard (2008) all examine AFDC participation and do not reach a consensus as to the effects of Medicaid expansions. Corson and McConnell (1990), McConnell (1991), Yelowitz (1996), and Shore-Sheppard (2008) examine SNAP participation, and generally find that Medicaid expansions increased SNAP participation. Two of these studies are closely related to mine. The first, Shore-Sheppard (2008), is the only other study to use a simulated instrument for Medicaid eligibility. In addition to AFDC and SNAP she also considers the effects of Medicaid expansions on rent assistance and SSI participation. She finds that Medicaid eligibility increases participation in every program except SSI, although these results were not robust to the inclusion of age-year fixed effects.¹¹

My study makes three primary contributions relative to Shore-Sheppard (2008) and other studies on program participation. First, my study is the first to estimate the effects of Medicaid expansions on two of the major components of the safety net targeting low-income families with children: EITC and TANF.¹²

no effect on TANF, SSI, or SSDI. Yelowitz (1998, 2000) finds a positive relationship between Medicaid and SSI participation.

¹¹Shore-Sheppard investigates crowd-out of private health insurance, but as a robustness check estimates the effect of Medicaid expansions on participation in the four safety net programs. She argues that the results being sensitive to the inclusion of age-year fixed effects is evidence of underlying within-age trends that drive large estimates of crowd-out of private health insurance in earlier work. Age-year trends may be problematic, but this specific test relies on the claim that Medicaid expansions should have no direct effects on program participation. However, the mechanisms outlined in the previous section provide plausible pathways for such direct effects. I further discuss age-year effects in Section 2.4 below.

¹²A single study, Brown et al. (2018), includes EITC receipts as an adult among their outcomes when considering the long-term effects of Medicaid. They find that Medicaid eligibility as a child reduces EITC receipt as an adult. This finding is complementary to mine, but I examine the effect of Medicaid eligibility on a child's family contemporaneous receipt of EITC. Many studies have considered TANF's cash-welfare predecessor, AFDC, but the programs differ in many respects and while I consider them jointly in my primary results, I find they are

Second, every prior study on Medicaid expansions and program participation has implicitly assumed that the eligibility rules for other safety net programs are not also being modified in ways that are correlated with Medicaid expansions. However, many safety net programs were also modified during this period. By simulating eligibility for a subset of outcome programs, my study is the first to test and control for this potential threat to identification. Finally, I decompose the effects on program participation into changes in take-up and changes in eligibility, rather than focusing purely on participation rates that may obscure larger, offsetting effects through take-up and eligibility.

The second study closely related to mine is Yelowitz (1996). Yelowitz estimates the effects of Medicaid expansions on SNAP participation and attempts to disentangle cross-program interactions due to the information and labor supply channels. He uses information in the Survey of Income and Program Participation (SIPP) regarding families' prior enrollment in safety net programs. Yelowitz argues that families previously enrolled in a safety net program are only affected through the labor supply channel and not the information channel, whereas families never enrolled in a safety net program are affected by both channels. He finds that information increases SNAP participation, but does not find any effect due to labor supply.

My method of decomposing take-up and eligibility mechanisms is distinguished from Yelowitz's in several ways. First, Yelowitz uses actual Medicaid eligibility, but actual eligibility is not exogenous with respect to demographic trends or economic conditions that also affect program participation. Second, Yelowitz's model implicitly assumes that the effect of Medicaid on SNAP participation due to labor supply is identical for families that previously participated differentially affected by Medicaid expansions when considered separately.

in SNAP and those who did not. However, the differences in SNAP participation history suggest that these families differ in other ways that may be related to labor supply decisions. My method of separating the take-up and eligibility channels does not require such an assumption because I estimate eligibility for SNAP. Finally, Yelowitz may isolate the effect of information on SNAP participation, but the residual coefficient on Medicaid eligibility does not necessarily estimate the effect due to labor supply. Effects due to other mechanisms, for instance stigma or transaction costs, may also be subsumed into this result. By estimating eligibility for SNAP and other safety net programs, I am able to explicitly separate changes in participation into changes in take-up and changes in eligibility. To my knowledge, my study is the first to produce such estimates of take-up and eligibility mechanisms.

1.3.2 Medicaid and Labor Supply

The second literature I contribute to studies the effects of Medicaid on labor supply. While numerous papers have studied the effects of children's Medicaid eligibility on labor supply, these works focus exclusively on the labor supply of single mothers. Strumpf (2011) and Decker and Selck (2012) exploit variation in the timing of implementation of Medicaid programs and find no evidence of a change in labor supply. Winkler (1991) and Moffitt and Wolfe (1992) study the early 1980s using variation in the value of Medicaid benefits across states. Winkler (1991) finds no effect on labor supply, while Moffitt and Wolfe (1992) finds a negative effect that is concentrated among families with high expected medical expenditures. A more recent set of papers study the Medicaid expansions that weakened the link to AFDC receipt. Montgomery and Navin (2000), Meyer

and Rosenbaum (2001), and Ham and Shore-Sheppard (2005) find no effect of Medicaid expansions on labor supply of single mothers. Yelowitz (1995) finds a positive effect on labor supply, but the same critiques from Ham and Shore-Sheppard (2005) mentioned in the discussion of AFDC participation apply here as well. Pohl (2018) uses a structural model to estimate the effects of more recent Medicaid expansions for children and forecast the effects of the ACA. He finds an imprecisely estimated positive effect. Broadly, there is a consensus that Medicaid expansions for children did not significantly affect the labor supply of single mothers.¹³

Overall, the existing literature finds that Medicaid did not significantly affect the labor supply of single mothers. I make two contributions relative to this literature. First, I expand the sample of families treated by Medicaid expansions from those headed by single mothers to all families with children. While families headed by a single mother are important, they comprise less than a third of the families with incomes in the range treated by Medicaid expansions and prior work has missed this margin on which labor supply may react to expansions. Second, even within single-mother families the prior literature has focused only on the labor supply of single mothers themselves. However, other family members may also modify their labor supply in response to Medicaid expansions, for instance older siblings still living at home. These family members may be much more responsive to a Medicaid expansion if single mothers

¹³There is a large literature on the effects of state-optional expansions of Medicaid to childless adults and the ACA on labor supply. Because they study a different set of expansions and focus on childless adults, I mention them only briefly. Baicker et al. (2014) study an expansion in Oregon and find no effect on labor supply. Garthwaite et al. (2014) find that a large disenrollment of Medicaid enrollees in Tennessee increased labor supply. DeLeire (2018), Ham and Ueda (2017), Agirdas (2016), and Dague et al. (2017) investigate other state-optional expansions and find mixed results. Studies examining the ACA expansions generally do not find evidence of effects on labor supply (Leung and Mas, 2016; Heim et al., 2017; Kaestner et al., 2017; Frisvold et al., 2018). A single paper considers the effects of expansions in Medicaid eligibility for pregnant women and finds a reduction in their labor supply (Dave et al., 2015).

are the primary source of child care for young children. By accounting for the labor supply of other family members, I am the first to allow for within-family spillovers in the effects of Medicaid expansion on labor supply.

1.4 Data

This paper uses the March CPS from survey years 1980-2012. The CPS collects information on program participation, income, and labor supply during the year prior to the survey. Thus, my data cover outcomes from 1979-2011. My sample encompasses the years right before the major expansions in Medicaid eligibility in the 1980s until just prior to the passage and implementation of the ACA. In principal, I could estimate Medicaid eligibility for years prior to 1980. However, the CPS does not ask recipients about program participation in surveys prior to 1980.

1.4.1 Program Participation Outcomes

I restrict my primary sample to families with children ages 0-17, although I use families without children for robustness checks. I code a child as receiving a program if any member of the child's family reports receiving benefits from that program because benefits are typically received and reported at the family level. Similarly, when considering the value of benefits received from each program I assign the total value of all benefits received by all family members to the child.

In my main analysis I consider the four programs that comprise the majority of the safety net targeting low-income families with children. The Supplemen-

tal Nutrition Assistance Program (SNAP, known as food stamps until 2008) provides in-kind nutritional assistance to low-income families. SNAP is the largest of the four safety net programs with expenditures of \$68 billion in 2017. The Earned Income Tax Credit (EITC) provides wage subsidies to low-income earners. The EITC is the second largest program, with 2017 expenditures of \$63 billion.¹⁴ Cash welfare includes two programs. Aid for Families with Dependent Children (AFDC) provided cash benefits to low-income, primarily single-mother families with children until 1996. After 1996, AFDC was replaced by a successor program, Temporary Assistance to Needy Families (TANF). I consider these two programs jointly as they provide the same transfer, namely cash. However, they differ in important respects, and so I also estimate the effects of Medicaid expansions on AFDC and TANF separately.¹⁵ TANF had total 2016 funding of \$16.5 billion from the federal government and an additional \$15 billion from states. Finally, rental subsidies covers a variety of programs that provide rent assistance to low-income families, the most important being programs implemented by the Department of Housing and Urban Development (HUD) with total 2017 funding of \$38 billion. SNAP and housing subsidies are in-kind rather than cash transfers. The CPS includes estimates of the value of these transfers to families, and I treat this value as a cash value in my analysis.¹⁶

¹⁴The CPS does not ask respondents about EITC benefits received, but rather imputes them. As a result, regressions of EITC receipt on Medicaid eligibility estimate the effect on EITC eligibility, not EITC participation.

¹⁵TANF is not an entitlement program, generally has more restrictive eligibility criteria, caps the number of years for which an individual may be enrolled in the program, and is funded through a block grant that is not adjusted for inflation. As a result, TANF benefits have eroded nearly 40% since its creation (Center on Budget and Policy Priorities, 2018).

¹⁶These estimates are the “face value” in the case of SNAP, treating the benefits as cash equivalent. Schanzenbach (2002) finds that families value food stamps at slightly less than face value. Hoynes and Schanzenbach (2009) find that households react similarly to a dollar in cash income versus a dollar from food stamps. Similarly, other in-kind transfers are valued at their market value, i.e. the total spending on recipients divided by the number of recipients. Using the face or market value of in-kind transfers may overstate the effect of the transfers on a families well-being, but more accurately reflects the change in program expenditures due to Medicaid

1.4.2 Labor Supply Outcomes

My primary labor force outcomes are total usual weekly hours worked and annual earned income. In my baseline analysis, I consider these outcomes for three groups: the entire family, adults 18 and over, and working-age teens within the family. Analyzing outcomes at the family level is important because families share resources and most programs assign eligibility at the family level. If Medicaid expansions cause intra-family spillovers in labor supply decisions, those spillovers would not be captured by analyzing individuals. The CPS asks labor force and income questions of all individuals 15 and older, thus by necessity my analysis of teen labor supply is constrained to the labor supply of children of these ages. Because income is recalled from the year prior to the survey, individuals as young as 14 report labor force and income questions in my sample.

1.4.3 Medicaid Eligibility

I construct two measures of Medicaid eligibility by applying eligibility rules for each state and year to the CPS sample. These rules cover income eligibility thresholds, ages of children, family structure, and parent's employment status.¹⁷ The first measure is actual Medicaid eligibility. In each CPS sample year I estimate the fraction of children eligible for Medicaid by state, child's age (0-17), and child's race based on the Medicaid rules effective in that year. Throughout, race is defined to be either non-Hispanic white (white) and all others (non-

expansions While these four programs represent the largest safety net programs specifically targeting low-income families with children, there are numerous other programs

¹⁷My eligibility calculations are based off code from Sarah Cohodes, Daniel Grossman, Samuel Kleiner, and Michael Lovenheim (2016), and I am thankful to them for sharing their code with me.

white). This measure captures variation in the actual, average level of children's eligibility for Medicaid in each age-race-state-year cell. State, age, and year are characteristics over which Medicaid eligibility varies explicitly. While Medicaid eligibility does not vary across race explicitly, in practice actual eligibility levels differ substantially between whites and non-whites due to differences in income and demographics. To the extent that these factors trend differently across races, failure to control for them will lead to biased estimates and recent literature using simulated Medicaid eligibility has found racial differences to be important (Dave et al. 2015; Cohodes et al. 2016). The CPS has large sample sizes, but when data are broken into age-state-race-year cells many cells are estimated off of small sample sizes, particularly in small, homogenous states. Thus, I estimate actual Medicaid eligibility as a three-year moving average of eligibility, as has been done in other recent studies using simulated Medicaid eligibility (Gruber and Simon 2008; Gross and Notowidigdo 2011; DeLeire, Lopoo, and Simon 2011; Cohodes et al. 2016).¹⁸

The actual Medicaid eligibility measure includes variation from confounding sources such as changing demographics, economic shocks, and family-level behavioral responses to changes in the policy environment, all of which may be correlated with program participation and labor supply. I follow previous literature and instrument for eligibility using a second measure of "fixed simulated eligibility" to retain only variation due to policy changes in Medicaid rules. To construct this simulated measure, I use the national 1990 CPS sample and estimate the fraction of this entire fixed sample that would be eligible for Medicaid in each state and year, separately by race and age. Income variables in the fixed sample are adjusted to each year using the CPI-U. In this way, the fixed sample

¹⁸My analysis covers the years 1980-2010, but I use data from 1979-2011 to compute the three-year averages.

does not vary across states or time, and all the variation in simulated Medicaid eligibility comes from changes in Medicaid rules. Because the sample is fixed, estimated eligibility cannot vary due to demographic trends, economic conditions, or changes in family labor supply decisions. This method was first used by Currie and Gruber (1996a,b) and Cutler and Gruber (1996), and has since been used widely in the Medicaid literature. See Brown et al. (2018); Levere et al. (2018); East et al. (2017) for recent examples and a brief review of literature using simulated Medicaid eligibility, and Paris (2018) for an example using simulated SNAP eligibility.

Figure 1.4 shows the average actual and simulated Medicaid eligibility measures overall and separately by race. Most of the aggregate variation in actual eligibility levels is explained by changes in Medicaid policy. All three pairs of series follow similar trends in aggregate from 1980 through the mid-1990s, but diverge between 1997-2000. Thereafter, the series follow nearly identical trends but with a persistent average gap between actual and simulated eligibility of about 6 percentage points, with the simulated eligibility measure being higher in all three cases. The timing of this divergence is worrying given the passage of the Personal Responsibility and Work Opportunity Act (PRWORA) in 1996 and CHIP in 1997. However, I apply the same Medicaid rules to both the simulated and actual samples, thus policy changes cannot account for the divergence between the two series. Instead, the late 1990s was a period of particularly strong real income growth. This growth is captured in the sample used to estimate actual Medicaid eligibility, but not by the fixed sample used for simulated eligibility which is adjusted for inflation using the CPI-U and has zero real income growth. After the strong real wage growth of the late 1990s families in the actual sample have higher family incomes than those in the fixed, simulated sample,

and thus a larger fraction of families exceeding eligibility thresholds in any year.

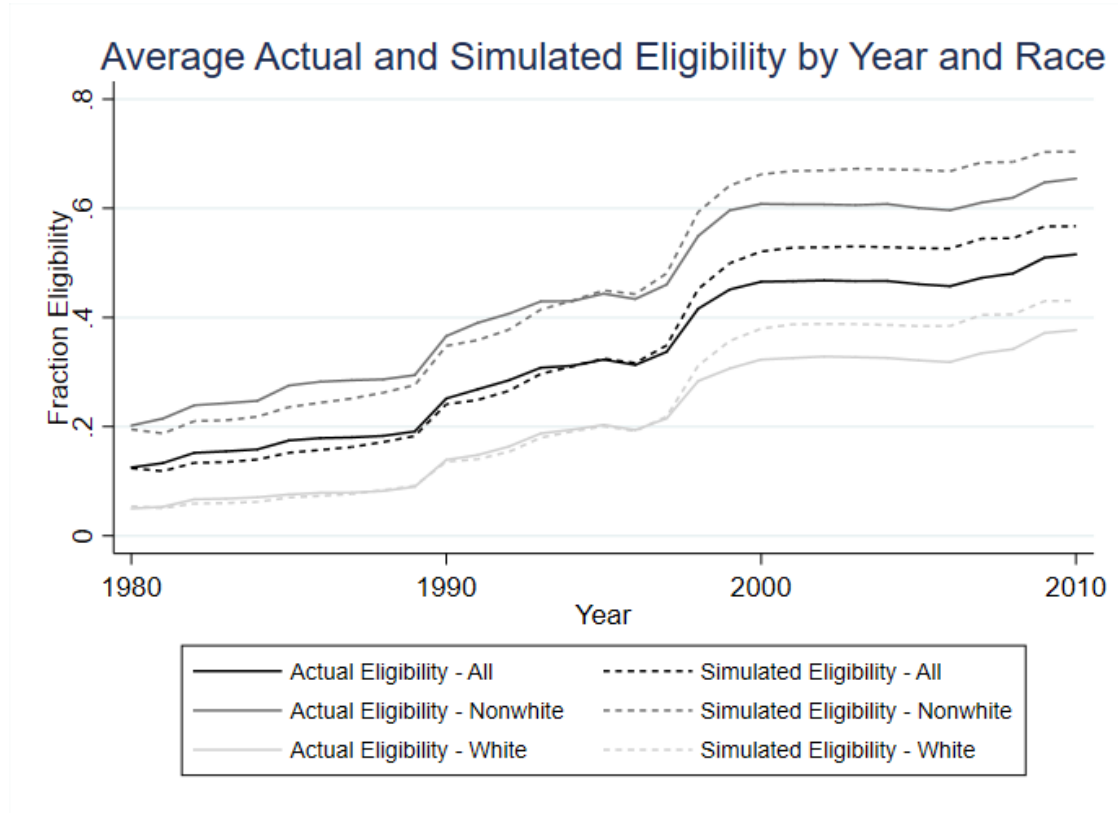


Figure 1.4: Author's calculations using the 1979-2011 March CPS and Medicaid eligibility rules for each state and year. Each line shows average eligibility for children ages 0-17, by race. Simulated eligibility uses a fixed sample of the 1990 CPS. Incomes for the fixed sample are adjusted to each year using the CPI-U.

In Appendix Section A.2 I show that nominal incomes in the actual and simulated samples trend similarly in all years except the latter half of the 1990s. The different trends between the samples in the late 1990s reflects the strong real wage growth during this period, a finding that has been previously found in the literature on income inequality. I then show that if I adjust incomes in the actual eligibility sample down by the ratio of average income in the simulated and actual samples in every year, the post-1996 gap in aggregate average eli-

gibility disappears entirely and the actual and simulated series trend similarly in aggregate throughout the entire period. This divergence in eligibility should not present an identification threat for my analysis as I include year fixed effects, and differences in eligibility due to different annual income growth rates will be absorbed.

My primary measure of Medicaid eligibility uses all changes to Medicaid eligibility rules, including optional expansions by individual states. This measure may not be exogenous if the timing of state-level Medicaid expansions is related to local demographic trends or economic circumstances. To address this concern I construct a second measure of Medicaid eligibility that uses only variation due to changes in minimum Medicaid eligibility rules required by federal law. Variation from federal rules comes from cross-state differences in pre-expansion AFDC generosity. Following Cohodes et al. (2016), I fix AFDC rules in 1980 and estimate the variation in eligibility that would have occurred if no states optionally expanded eligibility beyond federally required minimums. Policy changes at the national level are unlikely to be related to trends in specific states, making this federal expansion instrument more credibly exogenous. Figure 1.5 repeats Figure 1.4 for federal Medicaid eligibility.¹⁹ The federal eligibility measure shows the same pattern of divergence between actual and simulated eligibility in the late 1990s with parallel trends thereafter. As with overall eligibility, in Appendix Section A.2 I show that adjusting actual incomes by the ratio of average simulated and actual incomes in each year eliminates this gap.

Table 1.1 presents summary statistics of my analysis sample overall and separately by race. If there were no empty cells, I would have $18 \times 2 \times 51 = 1,836$ observations per year for 31 years, for a total of 56,916 observations. However, due

¹⁹Appendix Figure A.4 constructs the same graph using only state-optional eligibility.

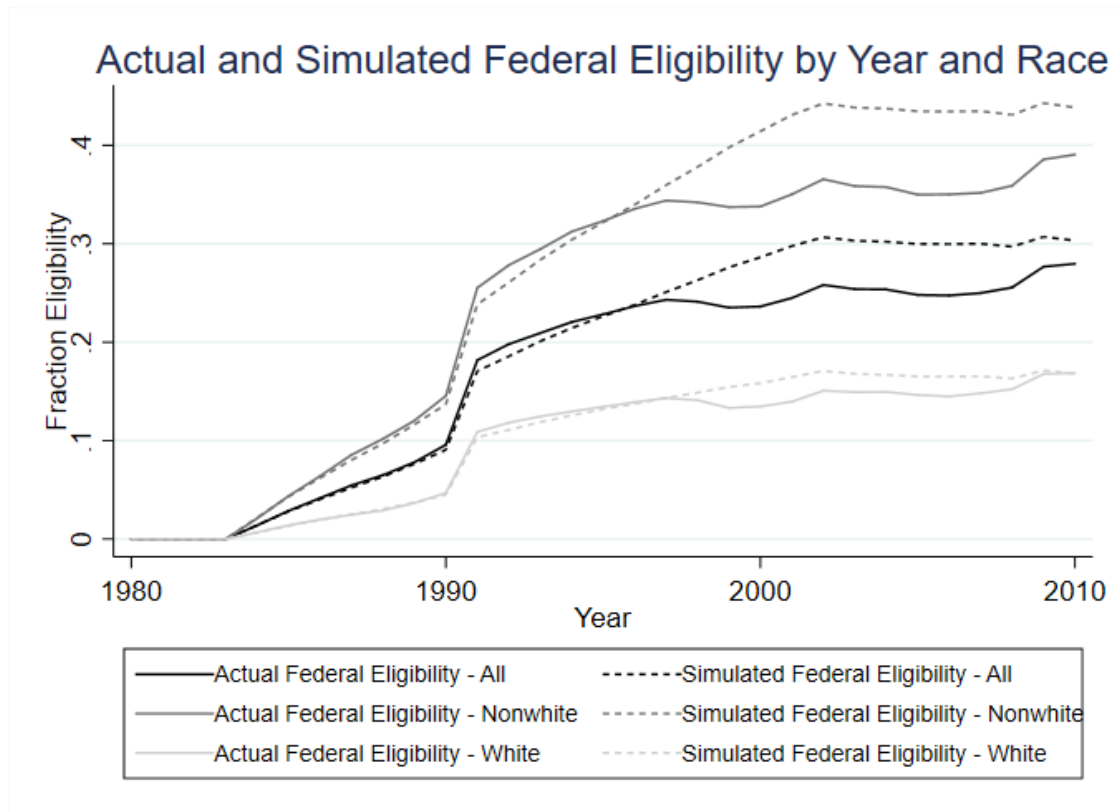


Figure 1.5: Author's calculations using the 1979-2011 March CPS, 1980 AFDC rules by state, and minimum federal Medicaid eligibility requirements by year. Each line shows average eligibility for children ages 0-17, by race. Simulated eligibility uses a fixed sample of the 1990 CPS. Incomes for the fixed sample are adjusted to each year using the CPI-U.

to sparse data in the CPS some cells are empty for small states with homogenous populations (e.g. there are very few non-white children in Vermont). As a result, my final analysis sample has 53,098 observations.²⁰ On average, families of non-white children have higher actual and simulated Medicaid eligibility, higher participation in other safety net programs, work less and have lower earned income, and have more (and younger) children. This pattern is expected given

²⁰When collapsing my analysis sample to cells, I use CPS person weights. The analysis sample is then weighted by the number of observations used to estimate each cell.

the strong correlation between race and income. These estimates do not compare directly to administrative counts of participants because I estimate participation rates in families with children, and not the entire population. However, they are similar to estimates from other sources. Shore-Sheppard (2008) finds similar shares of overall participation in AFDC, SNAP, rental assistance, and SSI. The difference in total family income across race is larger than measured in conventional Census estimates, but this is sensible because I consider only labor income which is skewed disproportionately toward white workers, while non-white families tend to receive relatively more income from transfer programs (Fontenot et al., 2018). My estimates of Medicaid eligibility are slightly higher than Cohodes et al. (2016), but this is expected because I consider a longer time period including more recent years when average eligibility levels were higher.

1.5 Methods

1.5.1 Empirical Methods

In an ideal setting with random assignment of Medicaid eligibility to children, program participation could be compared across families. If the timing of expansions and pre-expansion eligibility levels across states are unrelated to program participation outcomes, then variation in average eligibility from Medicaid expansions allows conditional random assignment. Thus, as an alternative to estimating the effect of Medicaid eligibility on individual program participation and labor supply outcomes, I estimate the effect of an increase in the share of children eligible for Medicaid on the share of children's families participat-

Table 1.1: Summary Statistics

	(1) All	(2) White	(3) Non-white
Age	8.95 (5.42)	9.03* (5.42)	8.77* (5.42)
White	0.70 (0.46)	1.00* (0.00)	0.00* (0.00)
# Children in HH	2.37 (0.40)	2.29* (0.35)	2.55* (0.45)
Family received any program	0.63 (0.19)	0.59* (0.19)	0.73* (0.16)
Total receipt, all programs	1,929.46 (1,788.47)	1,291.86* (936.87)	3,451.90* (2,334.96)
SNAP	0.14 (0.12)	0.08* (0.06)	0.25* (0.15)
Housing	0.06 (0.08)	0.03* (0.03)	0.12* (0.11)
Welfare	0.11 (0.10)	0.05* (0.05)	0.16* (0.15)
EITC	0.28 (0.15)	0.21* (0.10)	0.41* (0.16)
Total family labor income	71,781.82 (28,508.20)	83,675.18* (23,861.68)	43,383.10* (15,796.14)
Total family hours	52.80 (12.52)	57.36* (9.53)	41.93* (12.09)
Total family hours - adults	50.66 (11.20)	55.00* (7.82)	40.31* (11.26)
Total family hours - youths	2.14 (3.38)	2.36* (3.53)	1.62* (2.95)
All ages average eligibility	0.29 (0.21)	0.22* (0.15)	0.48* (0.21)
All ages simulated eligibility	0.32 (0.23)	0.25* (0.19)	0.50* (0.23)
N	52,183	27,626	24,557

Source: Author's calculations using the 1979-2011 March CPS. Sample is split between non-Hispanic whites and all others (non-white). Some statistics are estimated on a subset of the sample, for instance because labor force questions are only asked of those 15+ in the CPS. * indicates white and non-white samples differ at the $p < 0.05$ level.

ing in other safety net programs. Similarly for labor supply outcomes, I estimate the effect of an increase in the share of children eligible for Medicaid on average hours worked by each family, or average family incomes. By construction, variation in eligibility occurs the level for which Medicaid rules are simulated, that is age, race, state, and year, rather than by individuals. For all program participation and labor supply outcomes, I estimate models in the form of the following difference-in-differences framework:

$$Y_{arst} = \beta_0 + \beta_1 * Elig_{arst} + \beta_2 * \mathbf{X}_{arst} + \gamma_{rs} + \delta_{rt} + \lambda_{ar} + \theta_{as} + \epsilon_{arst} \quad (1.1)$$

where Y_{arst} is a family's program participation or labor supply outcome for a child of age a , race r (white or non-white), in state s , and year t . These outcomes are either the fraction of children whose families participate in a safety net program, their families average weekly hours worked, or average annual earned income. Medicaid eligibility is measured by $Elig_{arst}$, which estimates the fraction of children within each age-race-state-year cell who are eligible for Medicaid. Ordinary least squares estimates use actual Medicaid eligibility, while my primary estimates instrument for actual eligibility using simulated Medicaid eligibility to address the endogeneity concerns of actual eligibility. The coefficient of interest is β_1 , which estimates the change in the fraction of families participating in a safety net program in response to an increase in the fraction of children eligible for Medicaid. \mathbf{X}_{arst} are additional controls, which in my baseline regressions includes the state unemployment rate, number of children in the family, and simulated eligibility for the outcome safety net program being considered. All standard errors are clustered at the state level.

Before discussing identification, it is helpful to briefly discuss sources of vari-

ation in Medicaid eligibility, and highlight which sources I allow. Figure 1.6 shows variation in actual Medicaid eligibility by age and race. Younger children have higher levels of eligibility than older children, and non-white children have higher levels of eligibility than white children. I include age and race effects to remove these sources of variation. Trends across race also differ, and non-white children have an approximately 20 percentage point higher eligibility rate in 1980 that grows to 30 percentage points by 2010. To the extent that this growing gap is caused by differentially evolving income distributions across race, this variation in eligibility is unlikely to be exogenous and I include race-year fixed effects to remove it. Within age group, eligibility increases over time with large increases in particular years. These large within-age increases are a feature of the rules for Medicaid expansions. Earlier Medicaid expansions applied to children born after September 1983, leading to large within-age increases as these children aged. Similarly, CHIP expansions caused large within-age increases in eligibility in the years CHIP was created. I allow within-age variation in Medicaid eligibility in my estimation.

Figure 1.7 shows variation in Medicaid eligibility across the eight largest states by population. There is substantial variation in 1980 across states in eligibility levels due to differences in income distributions, demographic compositions, and levels of state AFDC generosity. In the late 1980s and early 1990s, this dispersion is compressed due primarily to federally mandated Medicaid expansions that forced relatively ungenerous AFDC states to raise their eligibility levels for Medicaid. This is the primary source of variation exploited by my federal measure of Medicaid eligibility. Because these expansions are due to federal policy changes, they are unlikely to be related to the circumstances of individual states. In 1997, CHIP is created and over the next several years

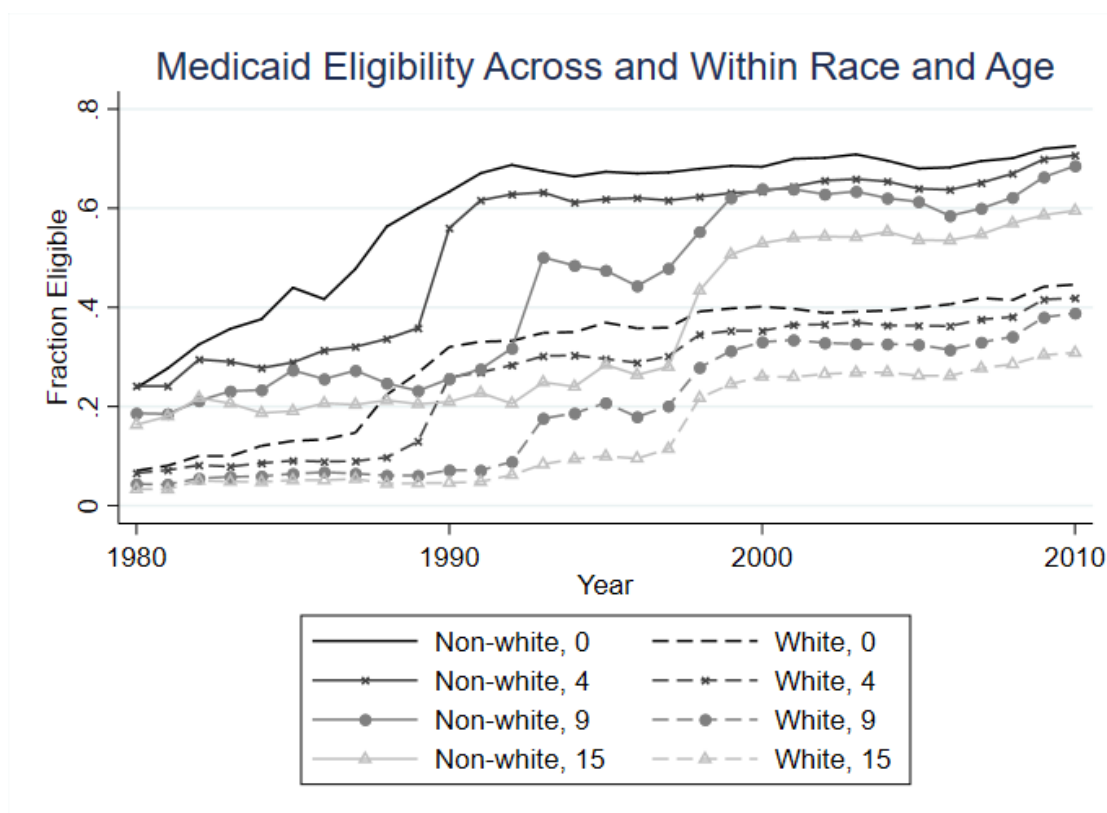


Figure 1.6: Author's calculations using the 1979-2011 March CPS and Medicaid eligibility rules for each state and year. Each line shows average actual eligibility for children of a specific age and race.

states implement CHIP programs. The timing of CHIP implementation, as well as the generosity of the CHIP programs, are both decided at the state level, and so are more likely to be related to state level demographic trends or economic characteristics. The variation due to these expansions will be included in my state-optional and overall Medicaid eligibility measures, but not my federal measure.

Because this is a difference-in-differences model, the two primary assumptions for identification are that Medicaid expansions are independent of any secular state trends in program participation or labor supply, and that there are

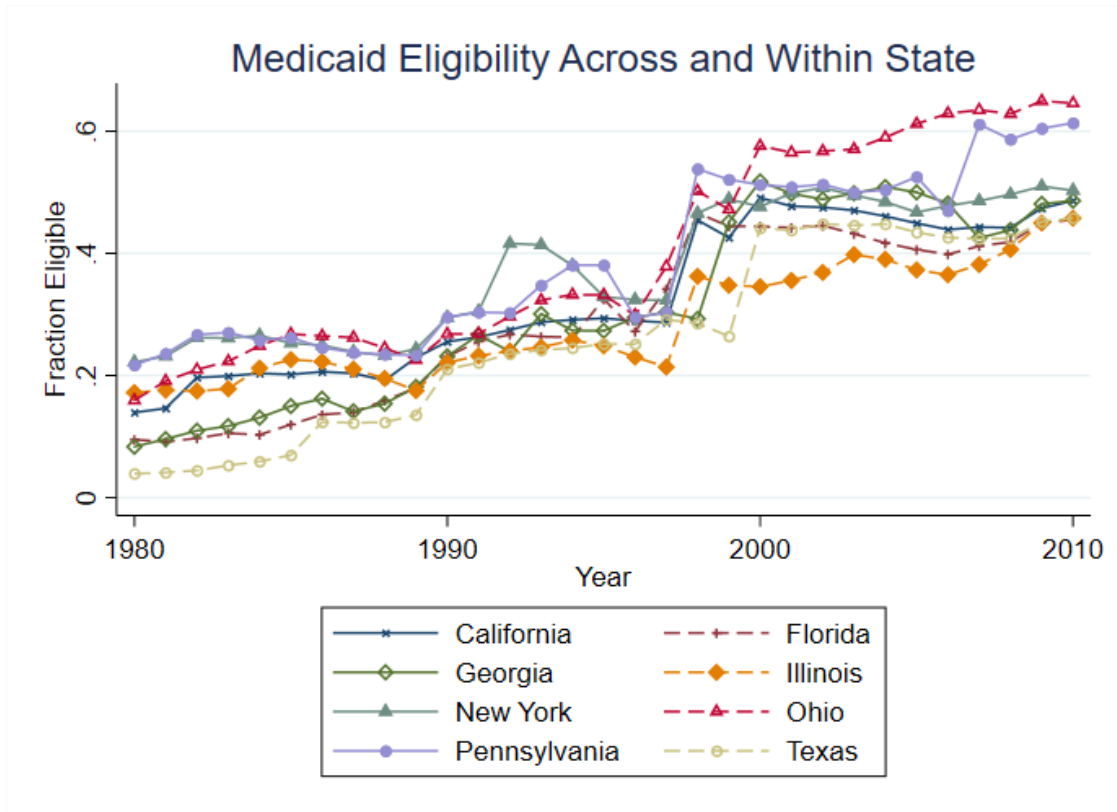


Figure 1.7: Author's calculations using the 1979-2011 March CPS and Medicaid eligibility rules for each state and year. Each line shows average actual eligibility for children of a specific age. Only eight states are graphed to reduce clutter, but the omitted states follow broadly similar patterns. The eight states included are the eight largest in terms of population.

no other contemporaneous policy changes that are correlated with Medicaid expansions and related to program participation or labor supply. Fixed effects are critical for addressing the first assumption and the identification of β_1 . A natural baseline model might include single fixed effects for the characteristics defining cells: age, state, race, and year. However, this model imposes unappealing assumptions, for instance that trends in Medicaid coverage were constant across race at the national level, and that white and non-white families are not differentially affected by economic shocks. These sources of variation are unlikely

to be exogenous due to differences of levels and trends in income distributions across race that are also related to participation in safety net programs.

These concerns motivate my inclusion of four of the six possible two-way interacted fixed effects in Equation 1.1. These more flexible fixed effects are race-state, race-year, age-race, and age-state fixed effects. Race-state effects allow for fixed differences across state and race that are related to Medicaid expansions and program participation. For instance, different income distributions, levels of parental educational attainment, or industrial structures across states that differentially affect white versus non-white families. Race-year effects allow different national-level trends by race such as differentially evolving income distributions or differential effects of economic shocks, both of which will be related to program participation and labor supply. Age-race effects allow for the coverage gap between white and non-white children to vary by age, which would be the case if younger children have younger parents, and white and non-white parents have different career trajectories. Age-state effects allow children of the same age but in different states to have fixed differences that are related to both Medicaid eligibility and program participation, for instance different industrial structures or labor markets.

This specification excludes age-year and state-year fixed effects. There may be variation in Medicaid eligibility due to trends within age or states that are related to Medicaid eligibility and program participation. However, my primary sources of identifying variation for overall Medicaid eligibility come from within-age and within-state changes in eligibility, and including age-year and state-year fixed effects excludes this variation. Card and Shore-Sheppard (2004) and Shore-Sheppard (2008) discuss the inclusion of age-year and state-year

fixed effects in models estimating the effects of Medicaid expansion so crowd-out of private health insurance. They find that the age-year effects matter substantially, reducing estimates by around 50%, while the state-year effects make little difference. In Appendix Tables A.2-A.7 I present results for a variety of alternatives to my preferred specification, including the inclusion of age-year and state-year effects. I find that in this context, the age-year effects in some cases reduce the precision of my estimates, and somewhat reduce the estimated magnitude of the effect of Medicaid expansions on labor supply (but not program participation). However, the inclusion of age-year effects does not change my qualitative results: federal Medicaid expansions increased participation in major safety net programs, while overall Medicaid expansions increased labor supply of families.

The second main assumption for identification is that there are no contemporaneous shocks or policy changes that are correlated with Medicaid expansions that would also affect program participation. The largest concern for this assumption is that eligibility rules for outcome safety net programs were changed in ways that were related to Medicaid expansions. If policy changes for outcome programs expanded (contracted) eligibility, and those changes were correlated with Medicaid expansions, then those policy changes would bias me towards finding a positive (negative) effect of the Medicaid expansions on program participation rates. SNAP, cash welfare, and the EITC all had major policy changes during this period that could potentially confound the effects of Medicaid expansions. To address this concern, I estimate fixed, simulated eligibility for the major safety net programs in the same manner as for Medicaid: I estimate the fraction of children in each age-race-state-year cell that were eligible for SNAP, cash welfare, and the EITC using the national CPS sample from 1990. In do-

ing so, I can explicitly test whether rules for outcome programs were modified in ways correlated with Medicaid expansions by using simulated eligibility for outcome programs as an outcome itself. In addition, I include simulated outcome program eligibility as a control when estimating the effects of Medicaid expansions on program participation. This ensures the estimated effects of Medicaid expansions are not due to contemporaneous changes to eligibility for the outcome safety net programs.

As a second method to address potential contemporaneous shocks or policy changes, I use only variation in Medicaid eligibility induced by changes in federally mandated rules, and exclude variation due to state-optional expansions. The variation exploited by this eligibility measure is due only to states having different pre-expansion AFDC policies. States are differentially treated to the degree the generosity of the AFDC policies differs from the uniform national coverage standards imposed by these federal expansions. As these expansions occurred at the federal level, they are unlikely to be related to state-level policies regarding Medicaid eligibility or rules for other transfer programs, or in response to local demographic or economic situations. Because the identifying variation for this federal measure relies on cross-state differences in pre-expansion AFDC generosity rather than within age or state increases in eligibility over time, the federal measure is also less likely to be susceptible to underlying trends mentioned for the first assumption, including underlying trends within age and state. To the extent these federal expansions are related to rules for other safety net programs administered at the federal level, the correlation between those rules is controlled for by my simulated eligibility for the outcome transfer programs.

Finally, I conduct several robustness exercises. I estimate models allowing for linear state time-trends, and find that these trends do not affect my estimates. I implement falsification tests by randomly assigning children between ages 0-17 to the subset of families in the CPS sample with no children. Because the expansions I consider are specifically for families with children, childless families should be unaffected, and I find no evidence of effects on the program participation and labor supply behavior of childless families. Even federal Medicaid expansions may conceivably be related to national economic conditions that differentially affect specific states, and I regress employment indicators on various measures of Medicaid eligibility to provide evidence that this is not a problem.

I also conduct, to my knowledge, the first explicit test of the exogeneity of the state-optional Medicaid expansions. Cohodes et al. (2016) informally test the exogeneity of state-optional expansions by comparing the effects of state-optional and federal Medicaid expansions on educational attainment. This comparison works for outcomes where the effects of Medicaid expansions are not heterogeneous based on family income, or work in the same direction. However, as shown in Figure 1.1 and by Goldsmith-Pinkham et al. (2018), federal Medicaid expansions are concentrated on low-income families, while state-optional expansions affected relatively higher-income families. As discussed in Section 1.2.2 and Figures 1.2 and 1.3, in my setting the effects of Medicaid expansions are potentially heterogeneous based on family income. Therefore, I compare the effects of federal Medicaid expansions to the effects of state-optional Medicaid expansions using a subset of state-optional expansion eligible children with incomes similar to those covered by the federal expansions. Reassuringly, I find similar results and largely unable to reject the null of equal effects for federal

and state-optional Medicaid expansions on program participation and labor supply. Overall, my results pass these robustness tests, giving credibility to my identification strategy and also providing support for the use of simulated Medicaid instruments in the literature more broadly, particularly instruments using state-optional expansions.

1.6 Results

1.6.1 Effects of Medicaid Expansions on Program Participation

Table 1.2 reports the effects of Medicaid expansions on participation in the major components of the safety net. Each cell is the estimate of a separate regression, and all estimates use the preferred instrumental variable specification of Equation 1.1 including the set of four two-way fixed effects. Row 1 uses variation in Medicaid eligibility from all expansions, including state-optional expansions, while Row 2 uses only variation in Medicaid eligibility from federally required expansions. Column 1 presents results from the first stage, which shows how a change in either measure of simulated eligibility for Medicaid affects actual eligibility. For overall Medicaid expansions, a 10 percentage point increase in simulated eligibility translates to a significant 8.0 percentage point increase in actual eligibility. An equivalently sized federal expansion is predicted to increase actual eligibility even more, by 10.5 percentage points.²¹

Columns 2-5 show the effects of Medicaid expansions on SNAP, cash wel-

²¹Throughout all specifications and outcomes, simulated eligibility remains a strong instrument for actual eligibility. My smallest F-statistics are around 30. In most specifications the F-statistic is near or above 100.

Table 1.2: The Effects of Average Medicaid Eligibility on Individual Program Receipt

	(1) First Stage	(2) SNAP Participation	(3) Welfare Participation	(4) Housing Participation	(5) EITC Eligibility
Panel A: Program Participation					
1. Overall Eligibility	0.800*** (0.022)	-0.016 (0.017)	0.028 (0.022)	0.008 (0.013)	-0.071*** (0.017)
% Effect of 10 p.p. Expansion	-	-1.2	2.5	1.4	-2.6
2. Federal Eligibility	1.046*** (0.052)	0.032*** (0.008)	0.051*** (0.010)	0.017*** (0.006)	-0.002 (0.009)
% Effect of 10 p.p. Expansion	-	2.3	4.6	3.0	-0.1
Panel B: Total Program Receipt					
3. Overall Eligibility	0.800*** (0.052)	-69.39 (91.91)	112.07 (172.40)	3.45 (4.11)	-169.93 (119.64)
% Effect of 10 p.p. Expansion	-	-1.0	1.3	1.4	-1.1
4. Federal Eligibility	1.046*** (0.022)	112.60*** (41.86)	211.75** (101.12)	6.33*** (2.26)	-5.37 (17.88)
% Effect of 10 p.p. Expansion	-	1.6	2.5	2.5	-0.1

Each cell is a regression of a different outcome on average Medicaid eligibility, where every regression uses the preferred IV specification including state-race, race-year, age-race, and age-state fixed effects. Panel A shows results for whether a child's family reported receipt of each transfer program. Panel B shows results for the total amount a child's family received from each program. Each panel splits analysis by both any source of Medicaid eligibility and only Medicaid eligibility from federally mandated expansions. All regressions control for state unemployment rates, number of children in the family, and simulated eligibility for the relevant outcome program. Alternative fixed effects specifications are available in the Appendix. Each regression has at most 53,098 observations. Standard errors are clustered at the level of the state. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively

fare, rental assistance, and EITC. The first three outcomes estimate the effect of Medicaid expansions on program participation, while the fourth estimates the effect on EITC eligibility. Estimates in Row 1 show that overall Medicaid expansions did not significantly affect participation in SNAP, cash welfare, or rental assistance, but did reduce eligibility for the EITC. A 10 percentage point expansion in overall Medicaid eligibility reduced EITC eligibility by 0.71 percentage points, or 2.3 percent relative to mean participation.²² This pattern of effects for overall expansions is unsurprising. A substantial amount of variation in overall Medicaid eligibility is due to state-optional expansions that affected relatively higher income families. For example, CHIP covers families with income up to 200 percent of the FPL in most states, and up to 300 percent in many states. Eligibility thresholds for SNAP, cash welfare, and rental assistance are comparatively low. SNAP's net income test is 100 percent of the FPL, while its gross income test is 130 percent of the FPL.²³ Prior to the major Medicaid expansions, the average AFDC eligibility threshold was only 60 percent of the FPL (Cutler and Gruber, 1996). TANF eligibility levels are less than 100 percent of the FPL in all but one state, with a majority of states having thresholds below 50 percent of the FPL (Falk, 2014). Housing programs have varied eligibility levels. Section 8 housing assistance requires families to have less than 50 percent of median income in their local area. Median household income in the U.S. was \$61,372 in 2017 (U.S. Census Bureau, 2018b). Using the national income distribution and a poverty threshold of \$25,100 for a family of four implies an eligibility threshold of 122 percent of the FPL. For comparison, in 2018 the EITC benefit for a family with two children phased out at \$45,802, or 182 percent of the FPL. Because

²²Appendix Table A.1 gives summary rates of participation, eligibility, and take-up for programs I consider.

²³Many programs, including SNAP, cash welfare, and Medicaid, use multiple tests with different thresholds.

low-income households tend to be geographically concentrated, actual eligibility thresholds for rent subsidies at the local level will be even lower in practice.

Row 2 shows the effects of federal expansions of Medicaid. A 10 percentage point expansion in Medicaid eligibility increases participation in SNAP, cash welfare and rental assistance, by 0.32, 0.51, and 0.17 percentage points respectively, or 2.3 percent, 4.6 percent, and 3.0 percent. Unlike the overall Medicaid expansions, the effects of federal Medicaid expansions are concentrated in low-income and single-mother families that have higher rates of eligibility for other safety net programs. Thus, it is similarly unsurprising that federal Medicaid expansions have stronger effects on program participation. Over the period 1980-2010, overall Medicaid expansions increased eligibility by 38.4 percentage points, and federal Medicaid eligibility increased 15.1 percentage points. This indicates that over my period of study, the overall expansions reduced EITC eligibility by 2.7 percentage points, and the federal expansions increased participation in SNAP, cash welfare, and rental subsidies by 1.2, 1.8, and 0.7 percentage points, respectively.

The pattern of results I find is consistent with several potential mechanisms. In the next section, I decompose eligibility and take-up mechanisms, and show that Medicaid expansions increased take-up for SNAP and cash welfare, but reduced eligibility. This indicates that the small, positive effects I find for program participation result from an increase in take-up, for instance due to applicants learning information about other safety net programs or facing lower stigma or transaction costs, and a reduction in eligibility, for instance due to an increase in labor supply. This explains why participation in SNAP, cash welfare, and rental subsidies increases, but EITC eligibility decreases. Results using OLS or

alternative specifications of controls may be found in Appendix Tables A.2 for overall eligibility and A.3 for federal eligibility. Appendix Table A.4 using only Medicaid variation for state-optional expansions. Note that federal and state-optional expansions are not mutually exclusive, and as a result the effects of federal and state-optional Medicaid expansions do not sum to the overall effect.

I control for simulated eligibility of outcome safety net programs to ensure the estimated effects of Medicaid expansions are not biased by contemporaneous policy changes for other programs. Panel A of Appendix Table A.8 shows the results of regressing simulated eligibility for outcome safety net programs on Medicaid eligibility as a test for correlation of changes to rules for outcome programs with Medicaid expansions. In general, I find that there is small amount of correlation between eligibility for safety net programs and Medicaid, and that it is generally not significant. Federal Medicaid expansions are not significantly related to eligibility rules of any outcome programs. Overall Medicaid expansions are significantly related to SNAP eligibility and weakly related to AFDC eligibility. Panel B of this table shows results for estimates of program participation when these controls are not included.

Comparisons of these results to prior literature have the caveat that previous literature on Medicaid expansions and program participation outcomes uses shorter periods of study, mostly years between 1987-1995, and different sources of variation in Medicaid eligibility. The largest federal expansions occur during these years, but the federal expansions are not completely phased in by 1995 and there are state-optional expansions in this period as well. Estimates from prior research are qualitatively similar to my estimates for federal expansions. Ham and Shore-Sheppard (2005) find no effect on AFDC participation, while

Shore-Sheppard (2008) find a statistically significant increase in the probability of AFDC participation of 0.83 percent.²⁴ Shore-Sheppard (2008) also finds that Medicaid expansions increased the probability of SNAP participation by 0.84 percentage points, and Yelowitz (1996) finds that Medicaid expansions increased SNAP participation by 0.22 percentage points. I find a somewhat larger effect, the overall Medicaid expansions increased SNAP participation by 0.53 percentage points. Overall, my estimates for program participation are consistent with the prior literature.

Panel B of Table 1.2 shows the effects of Medicaid expansions on the total benefits received from a program. These results are consistent with the effects on program participation. Overall Medicaid expansions do not significantly affect benefits received from any program, nor do overall expansions affect the benefits families are eligible to receive from the EITC. The estimated effect of expansions on EITC benefits is negative, consistent with the effect on EITC eligibility, but statistically insignificant. The effects of federal Medicaid expansions on total benefits are also consistent with the results for program participation. Federal Medicaid expansions increased average benefits for SNAP, cash welfare, and rental assistance, but did not affect benefits families were eligible to receive from the EITC. The 15.1 percentage point increase in federal Medicaid eligibility over this period indicates that total average benefits received per family increased by almost \$50, or around 3.1% as a result of federal expansions of Medicaid.

²⁴Shore-Sheppard (2008) estimates linear probability models on unaggregated participation outcomes rather than the aggregated fraction participating that I use, but the direction and magnitude of the estimates are similar.

1.6.2 Mechanisms: Take-up Versus Eligibility

Equation 1.1 measures the effect of Medicaid expansions on the fraction of the entire population participating in other safety net programs. However, the participation rate can be decomposed into program eligibility and program take-up conditional on eligibility, as shown in Equation 1.2:

$$ProgPart_{arst} = PElig_{arst} * Pr(Takeup|PElig)_{arst} \quad (1.2)$$

where $ProgPart_{arst}$ is the participation rate for a program, $PElig_{arst}$ is fraction of the population eligible for that program (not to be confused with the fraction eligible for Medicaid), and $Pr(Takeup|PElig)_{arst}$ is the take-up rate for the program, conditional on eligibility. I estimate $ProgPart_{arst}$ directly from the CPS data. I simulate eligibility for several major safety net programs to control for changes to program rules that are correlated with Medicaid expansions, and can use those same rules to estimate actual eligibility for outcome safety net programs $PElig_{arst}$, allowing me to back out the take-up rate $Pr(Takeup|PElig)_{arst}$. Taking the derivative of Equation 1.2 with respect to Medicaid eligibility gives:

$$\frac{dProgPart}{dMElig} = \frac{dPElig}{dMElig} * Pr(Takeup|PElig) + \frac{dPr(Takeup|PElig)}{dMElig} * PElig \quad (1.3)$$

In Equation 1.3, $\frac{dProgPart}{dMElig}$ is estimated from Equation 1.1 using program participation rates as an outcome, as reported in Table 1.2. Estimates of $\frac{dPElig}{dMElig}$ are similarly obtained using simulated actual program eligibility as an outcome. Thus, I can estimate $\frac{dPr(Takeup|PElig)_{arst}}{dElig}$, the effect of Medicaid eligibility on program take-up, as it is the only unknown quantity in Equation 1.3. In Table 1.3, I report

estimates of $\frac{dPElig}{dMElig} * Pr(Takeup|PElig)$, the net effect of eligibility mechanisms on program participation, and $\frac{dPr(Takeup|PElig)}{dMElig} * PElig$, the net effect of take-up mechanisms on program participation.²⁵

Rows 1 and 4 repeat the program participation results from Table 1.2. I replace housing subsidies and the EITC with AFDC and TANF separately, as overall and federal expansions have different effects for the different cash welfare programs. Rows 2 and 3 of Panel A report the net effects of the eligibility and take-up mechanisms on participation rates, respectively, with the percent effects of a 10 percentage point expansion of Medicaid eligibility reported below. Overall Medicaid expansions reduced eligibility for SNAP, cash welfare, and the NSLP. The reduction in participation due to changes in eligibility is particularly strong for SNAP and TANF; reduced eligibility for these programs decreased participation by 4.2 and 4.4 percent, respectively. In the case of TANF, this is primarily the result of the programs much lower base-participation rate. Only 4.5 percent of children live in families participating in TANF, compared to almost 14 percent for SNAP and 45 percent for the NSLP. The 0.57 percentage point reduction in SNAP eligibility (Row 2, Column 1) is larger than for cash welfare, but similar to the reduction for the NSLP of 0.81 percentage points. The larger eligibility effects for SNAP and NSLP are consistent with relatively higher income families, which are more likely to have two parents, having more flexibility in their labor supply. SNAP eligibility thresholds are substantially higher than those for cash welfare, and children are eligible for the NSLP until family income rises above 185 percent of the FPL. The percent change in NSLP

²⁵These results can only be estimated for the programs for which the CPS has participation data and for which I estimate eligibility using program rules. Of the primary safety net programs, this includes SNAP and cash welfare as the CPS does not contain data on EITC participation, and has insufficient data to estimate actual eligibility for rental subsidies. Because I cannot estimate take-up for the EITC and rental subsidies, I add the NSLP to Table 1.3.

Table 1.3: The Effects of Medicaid Expansions on Take-up and Eligibility

	(1)	(2)	(3)	(4)	(5)
	SNAP	Welfare	AFDC	TANF	NSLP
Overall Eligibility					
1. $\frac{d(ProgPart)}{dMElig}$					
% Effect of 10 p.p. Expansion	-0.016 (0.017)	0.028 (0.022)	0.028* (0.016)	0.005 (0.013)	-0.044** (0.022)
2. $\frac{d(PElig)}{dMElig} * Pr(Takeup PElig)$					
% Effect of 10 p.p. Expansion	-0.057*** (0.022)	-0.009 (0.018)	-0.011 (0.021)	-0.020*** (0.007)	-0.081*** (0.025)
3. $\frac{d(Pr(Takeup PElig))}{dMElig} * PElig$					
% Effect of 10 p.p. Expansion	0.041*** (0.016)	0.037* (0.021)	0.039*** (0.015)	0.025*** (0.009)	0.037* (0.020)
% Effect of 10 p.p. Expansion	3.0	3.3	2.5	5.6	0.8
Federal Eligibility					
4. $\frac{d(ProgPart)}{dMElig}$					
% Effect of 10 p.p. Expansion	0.032*** (0.008)	0.051*** (0.010)	0.007 (0.011)	0.074*** (0.014)	-0.017* (0.009)
5. $\frac{d(PElig)}{dMElig} * Pr(Takeup PElig)$					
% Effect of 10 p.p. Expansion	-0.048*** (0.014)	-0.013 (0.008)	-0.066*** (0.016)	0.027*** (0.010)	-0.023** (0.011)
6. $\frac{d(Pr(Takeup PElig))}{dMElig} * PElig$					
% Effect of 10 p.p. Expansion	-3.6	-1.2	-4.3	6.0	-0.5
Mean Participation Rate	0.137	0.111	0.154	0.045	0.455

Each cell estimates a different program participation, eligibility, or take-up outcome. Rows 1 and 4 repeat program participation outcomes from Table 1.2 for overall and federal Medicaid expansions, respectively. Rows 2 and 5 present results for the net effect of Medicaid expansions on program eligibility, and Rows 3 and 6 for the net effect on program take-up. Percent effects are given beneath each row, and are the net percent effect of the eligibility and take-up mechanisms on program participation. Robust standard errors for the effects on participation and eligibility (Rows 1, 2, 4, and 5) are clustered at the state level. Standard errors for the effect on take-up (Rows 3 and 6) are bootstrapped.

participation is smaller than for SNAP due to the NSLP having much higher participation rates. The reductions in eligibility are offset by increased take-up. SNAP and cash welfare take-up increase by 3.0 and 3.3 percent as a result of a 10 percentage point expansion in overall Medicaid eligibility. TANF increases by a larger 5.6 percent, although this is again reflective TANF's lower participation rate.

Panel B shows a similar pattern for federal Medicaid expansions. Federal expansions decreased eligibility for SNAP and cash welfare, although the reduction in cash welfare eligibility was driven by a reduction in AFDC eligibility. Both programs experience large increases in take-up than more than offset the reduction in eligibility. A 10 percentage point expansion increases take-up of SNAP and cash welfare by almost six percent each. Federal expansions lead to large increases in both take-up and eligibility for TANF, the only case where the effect of Medicaid expansions on program eligibility is positive. As a result, the overall effect on TANF participation is quite large, with a 10 percentage point expansion in Medicaid eligibility increases TANF participation by over 16 percent.

This pattern is consistent with the results from Table 1.2. Overall expansions had no significant effect on program participation because the eligibility and take-up effects approximately offset each other. However, the federal expansions significantly increased participation because the take-up effects overwhelmed the reductions in eligibility, which is unsurprising given that federal expansions are concentrated on families more likely to be eligible for other safety net programs. To put these estimates into perspective, between 1987 and 1995 total SNAP participation rose from 19 million to nearly 27 million, increas-

ing the participation rate from 7.9 percent to 10 percent. Large Medicaid expansions occurred during these years, and Yelowitz (1996) focused on this period to estimate the effects of Medicaid expansions on SNAP participation. The levels and change among participation rates of children in the CPS are larger as families with children tend to have higher rates of eligibility. Over this period, SNAP participation for children rose from 13.9 percent to 16.7 percent. The majority of federal expansions are implemented during these years, with federal Medicaid eligibility rising from around 10 percent to 21.2 percent.

The estimated effect of federal Medicaid expansions on participation of 0.032 implies that the rise in federal eligibility increased SNAP participation by 0.36 percentage points over this period, or 13 percent of the total increase in food stamp participation for children. Had eligibility remained constant, the estimated take-up effect implies that federal Medicaid expansions would have increased participation by 0.9 percentage points, or 32 percent of the actual increase in food stamp participation. Conversely, had take-up remained constant the estimate for reduced eligibility would have decreased participation by over 0.5 percentage points, or reduced the actual rise in food stamps by 19 percent. Thus, the estimated effects for take-up and eligibility have meaningfully large impacts on actual participation rates.

1.6.3 Effects of Medicaid Expansions on Labor Supply

Table 1.3 showed that Medicaid expansions caused reductions in eligibility for major welfare programs, particularly SNAP and cash welfare. Labor supply is an important mechanisms that might cause this reduction in eligibility for safety

net programs. It is also a mechanism I can examine directly, as measures of labor supply are captured in the CPS, unlike take-up mechanisms. As discussed in Section 1.2.2, the ex-ante effect of Medicaid expansions on labor supply is ambiguous but possibly positive. In Table 1.4, I estimate the effects of Medicaid expansions on two measures of labor supply: usual weekly hours worked and annual labor income. I estimate these two measures for families overall to account for potential within-family spillovers and separately by adults and teens within the family.

Using eligibility variation from all Medicaid expansions, estimates in Row 1 show that Medicaid expansions increased labor supply. A 10 percentage point increase in overall Medicaid eligibility increases usual weekly hours worked by 0.75, or 1.3 percent, for families overall, which is roughly split by increases for adults and teens of 0.38 and 0.37 hours, respectively. The effect on total annual labor income is consistent, albeit larger. A 10 percentage point expansion in Medicaid eligibility increases annual incomes for families by \$2,123, or 3.6 percent, with most of this effect being driven by increases in adult incomes. This effect is quite large. A potential explanation is that Medicaid expansions not only increases income on the extensive margin by family members working more hours, but also on the intensive margin if workers are able to switch to higher paying jobs.

A less benign explanation may be that overall Medicaid expansions are correlated with real income growth. This possibility is particularly concerning given that a large fraction of the increase in overall Medicaid eligibility occurs during the late 1990s due to CHIP expansions. However, these are the only years throughout the period 1980-2010 when real wages increased substantially

Table 1.4: The Effects of Expansions in Medicaid Eligibility on Hours Worked and Income for Families and Individuals

	(1) First Stage	(2) Total Family Hours	(3) Total Adult Hours	(4) Total Teen Hours	(5) Total Family Labor Income	(6) Total Adult Labor Income	(7) Total Teen Labor Income
1. Overall Eligibility	0.800*** (0.052)	7.521*** (1.761)	3.811*** (1.398)	3.711*** (0.503)	21227.25*** (7688.32)	20760.13*** (7720.90)	467.12*** (125.90)
2. Federal Eligibility	1.046*** (0.022)	0.609 (0.516)	0.110 (0.341)	0.499** (0.209)	5968.52 (5919.25)	5577.38 (5718.36)	391.14 (394.05)
Mean Dep. Var.	-	59,355	55,312	4,042	59651.02	58857.35	793.67

Each cell is a regression of a different outcome on Medicaid eligibility. Row 1 uses variation in Medicaid eligibility from all expansions. Row 2 uses only variation in Medicaid eligibility due to federally mandated expansions. Column 1 repeats first stage estimates. Columns 2-4 estimate the effect of Medicaid expansions on reported usual hours worked summed across all family members, for all adults 18+ within the family, and for all teens ages 14-17 within the family. While the CPS asks labor force questions of all individuals age 15+, the reference period is the previous year, when the youngest respondents were 14. Columns 5-7 estimate the effect of Medicaid expansions on total labor income for the entire family, adults, and teens. Each regression has at most 53,098 observations. Some have slightly fewer based on the number of empty cells. Standard errors clustered at the level of the state. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

in the population likely to be eligible for Medicaid, as discussed in Section 1.4.3. If states with strong wage growth in this period are also the states with the largest CHIP expansions, this would bias estimates of the effect on labor income in the positive direction and may explain the large effects on family and adult incomes. I return this issue in Section 1.8. However, note that if this were the case, it would bias the effects in the negative direction for program participation. Stronger wage growth would increase annual earned income but would also reduce eligibility for safety net programs and possibly reduce take-up for families facing stigma or high transaction costs of program enrollment.

The effects of overall Medicaid expansions on labor supply are largely consistent with the estimated effects on program participation and take-up from Tables 1.2 and 1.3. Overall Medicaid expansions reduced eligibility for the EITC, SNAP, and cash welfare. This reduction in eligibility is driven by an increase in labor supply. The increase in labor supply in response to Medicaid expansions has not been previously found in the literature, but this literature has looked almost exclusively at the labor supply of single mothers. In Table 1.5, I examine subsets of families treated by Medicaid expansions and similarly find no effect of Medicaid expansions when considering only the labor supply of single mothers. Additionally, these results for overall Medicaid expansions are robust to specification, as can be seen in Appendix Tables A.5-A.7.

Row 2 uses only variation due to federal Medicaid expansions, and does not find a significant effect of Medicaid expansions on labor supply. The estimates are precise enough to rule out more than a moderate effect on either usual weekly hours or annual income. A 10 percentage point expansion in federal eligibility increases an average family's usual hours worked and annual

Table 1.5: The Effects of Expansions in Medicaid Eligibility on Labor Supply by Family Structure

	(1) Single Mother Hours	(2) Single Mother Labor Income	(3) Single Parent Hours	(4) Single Parent Labor Income	(5) Two Parent Hours	(6) Two Parent Labor Income	(7) Other Adults Hours	(8) Other Adults Labor Income
1. Overall Eligibility	0.148 (0.600)	550.52 (416.38)	0.240 (0.759)	708.19 (496.32)	2.002 (1.833)	18776.51** (7916.98)	1.822*** (0.598)	1014.75 (1005.04)
2. Federal Eligibility	0.358 (0.346)	331.45 (261.05)	0.563* (0.338)	362.78* (200.83)	2.986*** (0.865)	2072.24** (889.47)	1.179*** (0.276)	1425.44 (930.97)
Mean Dep. Var.	7.056	3265.05	8.476	4292.57	41.93	32279.35	5.563	1755.15

Each cell is a regression of a different outcome on Medicaid eligibility. Row 1 uses variation in Medicaid eligibility from all expansions. Row 2 uses only variation in Medicaid eligibility due to federally mandated expansions. Pairs of Columns show the effect of Medicaid expansions weekly hours and annual income for single mothers, all single parents, all parents in two-parent families, and other adults living with families of all types. Each regression has at most 53,098 observations. Some have slightly fewer based on the number of empty cells. Standard errors clustered at the level of the state. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

income by 0.04 and \$88, or 0.1 percent for both outcomes, with similar scale estimates for both adults and teens. A 95 percent confidence intervals rules out positive effects of more than 0.3 percent for hours and 0.5 percent for annual income. The lack of an effect of federal Medicaid expansions on labor supply is consistent with the results from Table 1.2, but is at odds with the reduction in eligibility for SNAP and cash welfare found for federal expansions in Table 1.3. There are at least two explanations that may reconcile these results. First, other mechanisms such as changes in marriage or fertility may affect eligibility, although prior research has not evidence of large effects for these mechanisms. Second, the effects of expansions may be heterogenous across family types. For example, if the labor supply of single mothers, whose families comprise the majority of families covered by federal expansions, was relatively unresponsive to Medicaid expansions, but the labor supply for families with different structures was more responsive.

I provide some evidence for this second explanation in Table 1.5. Prior research has estimated the effects of Medicaid expansions for children only on the labor supply of single mothers, and broadly has found no significant effects. I am the first to broaden the analysis to include both additional family members beyond single mothers, and families with a structure other than a single mother. In Table 1.5, I estimate the effects of Medicaid expansions separately for single mothers, single parents, parents of two-parent families, and other adults living with a child's families that are not the child's parents. Similar to prior research, I find no significant effect of either overall or federal Medicaid expansions on the labor supply of single mothers. However, both federal and overall Medicaid expansions cause significant increases in labor supply for parents in two-parent families and other adults within families. This is consistent with research on

labor supply elasticities, which finds low elasticities for single mothers and relatively high elasticities for married women. Overall, Tables 1.4 and 1.5 provide evidence that Medicaid expansions did increase labor supply, and that the effects of within-family spillovers are significant.

1.7 Exogeneity of Federal and State-Optional Expansion

A major concern for state-optional Medicaid expansions is that the timing of implementation for these expansions may be related to state-level demographic or economic characteristics. While federally mandated expansions of Medicaid are unlikely to be related to state-level characteristics, the treatment intensity of individual states is still a function of the generosity of pre-expansion state-level AFDC policies. Thus, even federal Medicaid expansions might be indirectly related to state-level economic circumstances if local AFDC policies are related to local economic conditions. I test the exogeneity of the measures of Medicaid eligibility by regressing local labor-market indicators on Medicaid eligibility, including the state labor force participation rate, unemployment rate, and employment-population ratio. These results are shown in Table 1.6. For overall and federal expansions, I do not find any significant relationship between expansions and contemporaneous labor force indicators, corroborating that expansions do not appear to be motivated by local economic circumstances.

As shown in Figure 1.1 and by Goldsmith-Pinkham et al. (2018), federal and state-optional Medicaid expansions were concentrated in different parts of the income distribution. The effects of the two types of expansions may differ because variation from state-optional expansions is not exogenous, but also due

Table 1.6: Test for Correlation Between Expansions and Labor Force Indicators

	(1) State Labor Force Participation Rate	(2) State Unemployment Rate	(3) State Employment- Population Ratio
1. Overall Eligibility	1.250 (0.933)	0.007 (0.013)	1.148 (0.893)
% Effect	0.2	0.0	0.2
2. Federal Eligibility	0.524 (0.523)	0.003 (0.008)	0.481 (0.497)
% Effect	0.1	0.0	0.1
Mean Dep. Var.	66.712	6.021	62.735

Each cell is a regression of a different state labor force indicator on average Medicaid eligibility, where every regression uses the preferred IV specification including state-race, race-year, age-race, and age-state fixed effects. Panel A shows results for whether a child's family reported receipt of each transfer program. Row 1 uses variation from overall Medicaid expansions. Row 2 uses only variation from federal expansions. All regressions control for state unemployment rates, number of children in the family, and simulated eligibility for the relevant outcome program. Each regression has at most 53,098 observations. Standard errors clustered at the level of the state. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively

to heterogeneous effects based on incomes of affected families. Some state-optional expansions extended coverage to children whose families' incomes were similar to children covered by federal expansions. Examples include state-optional expansions that occurred prior to federal expansions, covered children in low-income families with children too old to qualify for federal expansions, or covered children whose family had a structure that qualified under a state-optional expansion but not a federal expansion. To test whether the effects of federal and state-optional expansions differ due to heterogeneous incomes of covered families, I select the subset of children covered by state-optional expansions who are ineligible for federal expansions but whose families' incomes meet at least one income test for federal eligibility. Figure 1.8 shows average eligibility by the income percentile of children's families for the federal expansions and this low-income subset of state-optional expansions in the year 2000. A much smaller fraction of children are eligible for the state-optional expansions than the federal expansions, but the incomes of the state-optional eligible children fall in a similar range to those eligible for federal expansions, indicating that the two groups are more comparable with respect to family income.

In Table 1.7, I re-estimate the effects of Medicaid expansions on program participation and labor supply outcomes using the low-income subset of children eligible for state-optional expansions. The last row of each panel contains p-values for a test of equality of the federal and state-optional coefficients. Only for housing participation and teen labor supply outcomes do I reject the null of equality between estimates. Due to the construction of the low-income state-optional sample, the cross-sample comparison of effects on teen labor supply is the most likely to be problematic. This is because a significant portion of the children eligible for state-optional expansions whose families have low in-

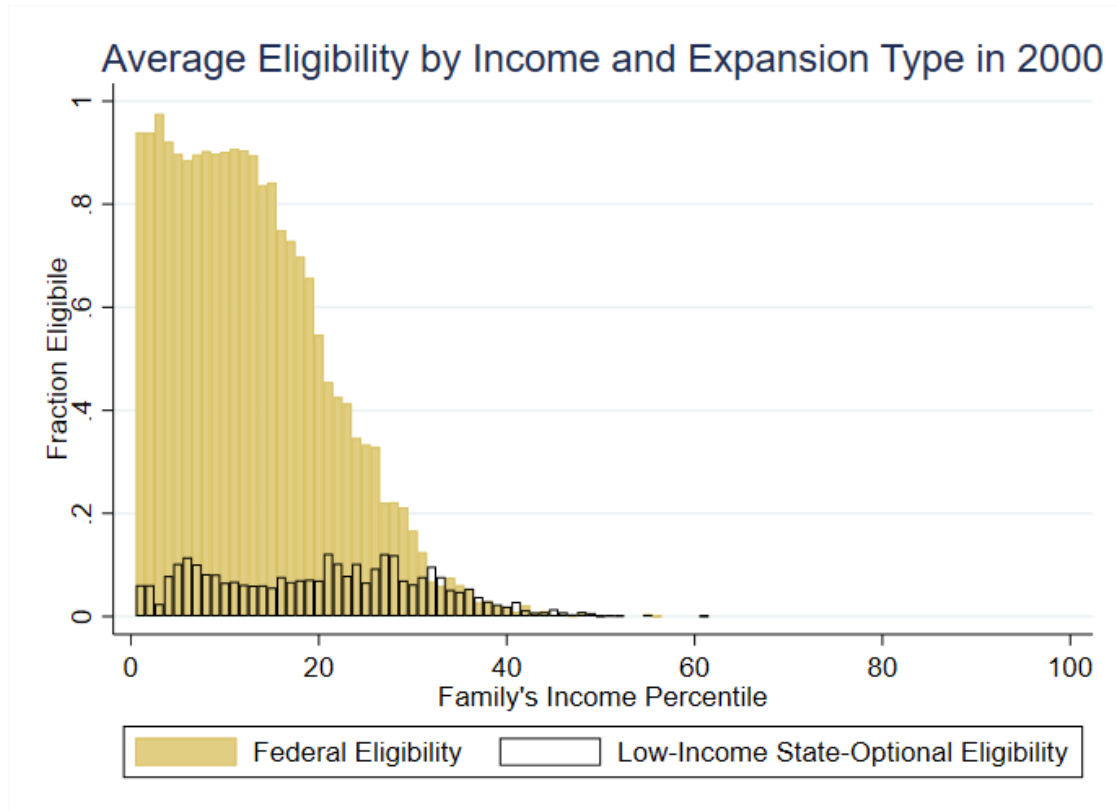


Figure 1.8: The figure shows the fraction of children age 0-17 eligible for federal and state-optional Medicaid expansions by the income percentile of the children's families in the year 2000. The state-optional eligible population is constrained to children whose families meet at least one income test for federal Medicaid eligibility to select a subsample with similar incomes. Federal eligibility uses only federal Medicaid rules holding AFDC rules fixed to 1980. State-optional expansions include all state expansions to Medicaid and CHIP. Income percentiles are determined using families' gross income.

comes are those who pass federal Medicaid income tests, but were born before September 1983. As a result, children in the state-optional sample are systematically older, and are necessarily from states that optionally provide more generous coverage to older children. While this low-income state-optional subsample is more comparable the federal sample than the overall state-optional sample, the significant difference of teen incomes reflects the fact that these samples are not perfectly comparable. Because the major difference between the samples is along the dimension of teens born before and after September 1983, it is not surprising that direct comparisons on outcomes for this group find significant differences. The systematic difference in teens between the samples also matters for program participation and family labor supply, but the effect is muted because teens comprise a small fraction of families' total incomes. Overall, these results suggest that the exogeneity of state-optional Medicaid expansions is not problematic for family-level outcomes.

1.8 Robustness

I conduct several robustness checks of my results, primarily focused on testing the primary identification assumption that there are no secular trends related to both Medicaid expansions and program participation that might drive my results. In Table 1.8, I re-estimate the results from Tables 1.2 and 1.4 for program participation and labor supply with a model that includes state-specific linear trends as controls. If the effects of Medicaid expansions were driven by underlying trends related to program participation and Medicaid expansions, then including state-specific linear time trends would substantially affect results. As shown in Panel A, this is not the case. The inclusion of linear trends increases

Table 1.7: Testing the Exogeneity of State-Optional Medicaid Expansions

Panel A: Program Participation					
	SNAP	Welfare	Housing	EITC	
1. Federal Eligibility	0.032*** (0.008)	0.051*** (0.010)	0.017*** (0.006)	-0.002 (0.009)	
2. State-Optional Eligibility	0.028 (0.023)	0.038 (0.040)	-0.063** (0.029)	-0.011 (0.034)	
P-Value For Test of Equality	0.541	0.195	0.000	0.287	
Panel B: Labor Supply					
	Total Family Hours	Total Adult Hours	Total Teen Hours	Total Adult Labor Income	Total Teen Labor Income
3. Federal Eligibility	0.609 (0.516)	0.110 (0.341)	0.499** (0.209)	5968.52 (5919.25)	391.14 (394.05)
4. State-Optional Eligibility	0.885 (0.665)	0.182 (0.455)	0.703*** (0.251)	7688.33 (7919.07)	665.06* (403.53)
P-Value For Test of Equality	0.457	0.606	0.004	0.465	0.009

Each cell is a regression of a different outcome on Medicaid eligibility. Rows 1 and 3 use variation from federal Medicaid expansions. Rows 2 and 4 use a subset of overall Medicaid eligibility coming only from state-optional expansions to low-income families. These families are selected as families with children who are not eligible for Federal expansions, but meet at least one income test for federal eligibility. Panel A re-estimates the results from Table 1.2 using these state-optional expansions, Panel B repeats the estimates from Table 1.4. The bottom of each panel shows p-values for a test of equality between the federal and state-optional estimates. Standard errors clustered at the level of the state. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

the magnitude and significance of the effect of overall Medicaid expansions on cash welfare and slightly reduces the estimated effect on EITC eligibility. The now statistically significant effect on cash welfare aside, the qualitative effects of the overall expansions are unchanged. State linear time trends are even less important for federal expansions, and the estimates are nearly unchanged. For program participation, there is little evidence of problematic underlying trends.

Panel B shows results for labor supply outcomes with the inclusion for state linear time trends. Results for hours, in Columns 1-3, are largely unaffected by the inclusion of state trends. Row 3 shows that overall Medicaid expansions are still estimated to have a positive effect on hours worked. Row 4 finds a statistically insignificant but positive effect of federal expansions on hours worked. However, state linear time trends are important for labor income outcomes. Focusing on the effect of overall expansions on family income (Row 3, Column 7), a 10 percentage point increase in overall Medicaid eligibility increases total family income by \$778, or 1.3%. This is the same increase seen in total family hours in Table 1.4, and much smaller than the estimated 3.6% increase in total family income without state linear time trends. This suggests that there are secular state-level trends positively related to Medicaid expansions and family labor income, with a likely candidate being the substantial real wage growth during the late 1990s. Despite the attenuated estimates for family income, including state linear time trends corroborates the estimated positive effect of overall Medicaid expansions on family labor supply. Linear state trends do not qualitatively change the estimated effect of federal Medicaid expansions on labor income.

As a falsification test, I use the subset of families in the CPS that are childless. Because I consider only Medicaid expansions for children, the program partic-

Table 1.8: The Effects of Medicaid Expansions on Program Participation and Labor Supply Including State Linear Trends

Panel A: Program Participation		(1)	(2)	(3)	(4)	(5)	(6)	(7)
	First Stage		SNAP	Welfare	Housing	EITC		
1. Overall Eligibility	0.893*** (0.036)	0.019 (0.014)	0.050*** (0.015)	0.009 (0.010)	-0.054*** (0.017)			
2. Federal Eligibility	1.042*** (0.023)	0.031*** (0.008)	0.047*** (0.010)	0.018*** (0.006)	-0.013 (0.009)			
Mean Dep. Var.	-	0.137	0.111	0.056	0.276			
Panel B: Labor Supply		First Stage	Total Family Hours	Total Adult Hours	Total Teen Hours	Adult Labor Income	Teen Labor Income	Total Family Labor Income
3. Overall Eligibility	0.893*** (0.036)	6.406*** (1.288)	2.418** (1.060)	3.989*** (0.420)	7191.66*** (2323.88)	583.41*** (114.59)		7775.06*** (2334.32)
4. Federal Eligibility	1.042*** (0.023)	0.697 (0.754)	0.681 (0.733)	0.016 (0.285)	1088.78 (1056.00)	-81.58 (106.89)		1007.20 (1053.71)
Mean Dep. Var.	-	59.355	55.312	4.042	58857.35	793.67		59651.02

Each cell is a regression of a different outcome on Medicaid eligibility. Rows 1 and 3 use variation from overall Medicaid expansions. Rows 2 and 4 use variation from federal Medicaid expansions. Panel A re-estimates the results from Table 1.2 using these state-optional expansions, Panel B repeats the estimates from Table 1.4. Each cell now includes state linear time trends. Standard errors clustered at the level of the state. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

ipation and labor supply behavior of childless families should not be affected by these expansions. Appendix Table A.9 shows these results for program participation. The estimated effects of Medicaid expansions on these families are nearly all precisely estimated zeros. The small coefficients partly reflect the general lack of generosity of safety net programs towards childless families in the U.S.; few of these families would be eligible to these programs to begin with. However, even among the programs which these families are more likely to be eligible for, SNAP and EITC, the effects are not statistically significant.

Table A.10 estimates an analogous test for labor supply outcomes, separately by family structure as in Table 1.5. Unlike program participation, childless adults participate in the same labor market as families with children, and so if there were underlying labor market trends related to Medicaid expansions, then the effects of Medicaid expansions for children should be significantly related to labor supply outcomes of childless families. I find little evidence that this is the case. The estimated effects of Medicaid expansions are statistically insignificant for nearly all outcomes, and the point estimates are generally small in magnitude. Overall, these robustness checks provide evidence that my results are driven by Medicaid expansions and not by any underlying trends related to Medicaid expansions and program participation and labor supply.

1.9 Discussion

In this paper, I provide the first evidence on the overall effects of expansions of public health insurance to families with children on program eligibility and take-up. In addition, I provide the first estimates of the effects of these ex-

pansions on family labor supply, rather than the labor supply of only single mothers. Overall Medicaid expansions do not significantly affect program participation, but a 10 percentage point federally mandated expansion increases participation in SNAP, cash welfare, and rental assistance by 0.3, 0.5, and 0.2 percentage points, respectively. These modest effects on program participation obscure larger but offsetting effects on program take-up and program eligibility. Both overall and federal Medicaid expansions increase program take-up among eligibles but reduce overall eligibility. I provide evidence that these reductions in eligibility are driven by increases in labor supply in response to Medicaid expansions, particularly for the overall Medicaid expansions. Prior research has not found evidence of these effects on labor supply, and I show that this is because this research has focused solely on single mothers for whom I also find no significant labor supply responses. Instead, labor supply responds among two-parent families and other working adults in all families.

I show that there is evidence of expansions to SNAP and AFDC that are correlated with Medicaid expansions by simulating eligibility for these programs. Including a measure of simulated eligibility for these programs controls for contemporaneous changes to program rules that are a threat to identification. These controls are relevant to the broader literature studying the effects of Medicaid because many outcomes of interest will be affected by SNAP participation, in addition to Medicaid. For instance, health or developmental outcomes are also improved by SNAP. Papers studying these outcomes that do not control for SNAP expansions may overestimate the effect of Medicaid itself on these outcomes, as the overall effect of Medicaid expansions is also driven by increased SNAP take-up. Finally, I provide evidence corroborating the credibility of estimates produced using state-optional Medicaid expansions.

My results have several important implications for policy discussions. First, the lack of coordination between transfer programs reduces participation insofar as families have incomplete knowledge of available programs and eligibility requirements. Increasing coordination in program implementation could increase program participation while simultaneously reducing administrative costs. Second, to the extent that Medicaid expansions increase labor supply, the costs are reduced by increased tax receipts. Overall Medicaid eligibility increased by 38.4 percentage points from 1980 to 2010. The estimated effect of this expansion on average family income when including state linear-trends as a control is $0.384 * 7,775 = \$2,986$ (Table 1.8, Row 3, Column 7). There are currently 34 million families in the U.S. with children. Assuming they pay a 15 percent marginal rate, this implies overall Medicaid expansions increased tax revenue by $2,986 \times 34,000,000 \times 0.15 \approx \15 billion. Finally, a caveat to Medicaid's expansionary effects on labor supply is that the increase may be primarily due to relaxing the distortions caused by the program's own strict discrete eligibility design rather than some inherently pro-labor effect of Medicaid. If so, further expansions may not reproduce this increase in labor supply.

CHAPTER 2

**THE EFFECTS OF SOCIAL SECURITY INCOME ON HEALTH CARE
EXPENDITURES AMONG THE ELDERLY**

2.1 Introduction

Between 1960 and 2017 health care expenditures in the United States rose as a share of GDP from 5.0 to 17.9 percent (Centers for Medicare & Medicaid Services, 2018). Major factors that contribute to the rapid increase in health care expenditures include rising income, improving medical technology, worsening health, demographic changes, and expanding health insurance coverage (Smith et al., 2009). Understanding the mechanisms behind rising health care expenditures is important for several reasons including forecasting future expenditures, evaluating whether current expenditures are the result of welfare optimizing decisions by consumers or caused by inefficiencies such as externalities or monopoly providers, and understanding how changes in policy, income, and health care technology affect individuals or households. Given that real GDP per capita has more than tripled since 1960 (Federal Reserve Economic Data, 2018), it is possible the increase in expenditures was driven by rising incomes. Thus, it is important to understand how income affects household consumption of health care.

Estimating the effect of income on health care expenditures is difficult due to the endogenous relationship between household income and health. Income may increase health care expenditures if consumers invest in their stock of health (Grossman, 1972). However, a body of research has found that reductions in income due to recessions actually reduce mortality, although more re-

cent work has found that this relationship has faded and that some forms of mortality are counter-cyclical (Ruhm, 2000; Neumayer, 2004; Tapia Granados, 2005; Gerdtham and Ruhm, 2006; Buchmueller et al., 2007; Gonzalez and Quast, 2011; Ariizumi and Schirle, 2012; McInerney and Mellor, 2012; Ruhm, 2015; Van Den Berg et al., 2017). Health can also directly affect income. For instance, being obese can reduce wages (Cawley, 2004). Chronic joint pain reduces labor force participation and wages (Garthwaite, 2012). Other factors such as genetics may affect both income and health (Smith, 1999). As a result, most research on the link between income and health have used empirical methods that do not estimate plausibly causal estimates that would be necessary to understand household behavior.

In this paper, I study how exogenous variation in income affects expenditures on health care among elderly households. The specific parameter this paper estimates is the income-elasticity of total out-of-pocket health care expenditures in elderly households in response to an increase in Social Security income.¹ I estimate this effect by exploiting variation in Social Security income across birth cohorts unintentionally introduced by Congress in amendments to the Social Security Act in 1972 and 1977 known as the Social Security benefits “Notch.” Variation in Social Security benefits across birth cohorts was driven by interactions between the amendments and high rates of inflation throughout the 1970s, which created large and unanticipated differences in benefits across otherwise similar birth cohorts. The Notch has been previously used to study a variety of outcomes including elderly health care expenditures (Tsai, 2018), prescription drug use (Moran and Simon, 2006), labor supply (Krueger and Pis-

¹Throughout, Social Security refers specifically to Old Age and Survivors Insurance (OASI). Benefits for recipients of Social Security Disability may have also been affected by the legislative changes I study, but I do not consider those recipients.

chke, 1992; Vere, 2011), elderly living arrangements (Engelhardt et al., 2005), mortality (Snyder and Evans, 2006), obesity (Cawley et al., 2010), formal and informal home care use (Shah Goda et al., 2011; Tsai, 2015), and cognitive function (Ayyagari and Frisvold, 2016).

While studying the effects of income on health care spending is of general interest, expenditures among the elderly is of particular interest. Medicare was the single largest purchaser of personal health care in 2016, totaling 22 percent of all expenditures. Because Medicare composes 65 percent of all elderly health care expenditures this implies the elderly make up over a third of total health care expenditures. Despite the recent slowdown in the growth of expenditures from both public and private insurance, the rate of annual increase of Medicare expenditures is expected to rise to six or seven percent. This expected increase is due to per capita expenditures increasing by four percent annually combined with demographic shifts due to retiring baby boomers (MedPAC, 2018; Board of Trustees, 2018). As a result, Medicare expenditures will continue to rise as a share of GDP. While overall health care spending has more than doubled since the 1970s, Medicare expenditures have more than tripled (MedPAC, 2018). Thus, rising expenditures among the elderly are an important factor in the rising share of health care costs as a share of GDP overall.

Over the same period, real median household income in elderly households has increased around 130 percent (U.S. Census Bureau, 2018a). This suggests that rising incomes among the elderly may potentially explain a significant share of the increase in expenditures. However, the prior literature on the income-elasticity of health expenditures has tended to estimate national or cross-national elasticities. The best-known estimates using individual data from

the RAND health insurance experiment explicitly exclude anyone over 62 from their sample (Manning et al., 1987), despite the relevancy of health care expenditures among the elderly to public policy.

Finally, understanding income and health expenditures among the elderly is important not only because of the growth in expenditures, but also because the elderly are often exposed to high out-of-pocket expenditures and significant financial risk, particularly among the poor. While the elderly receive significant assistance through public insurance which covers around 65 percent of their total costs, nearly 20 percent of their expenditures are financed out-of-pocket (De Nardi et al., 2016). These out-of-pocket expenditures expose the elderly to considerable financial risk despite the availability of public health insurance (Goldman and Zissimopoulos, 2003; Marshall et al., 2010). Understanding the health care purchasing behavior of elderly households is important to effectively designing public policy to address this financial risk and to identify for which services elderly households need additional support.

Using households where the primary Social Security beneficiary has a high school education or less as a proxy for low-income households, I find that health care expenditures among the low-income elderly are highly elastic. Income elasticity estimates range from 0.98 to 3.76 depending on the subsample, with a preferred estimate of 2.56. A \$1,000 increase in Social Security income, which is equal to approximately half of the increase in benefits low-education households received as a result of the Notch, would increase out-of-pocket expenditures on health care by \$200 in the average household. Elderly households that retired at a younger age have more elastic demand for health care than older retirees, suggesting poor health may play a role in their retirement. In addition, I

find evidence that larger Social Security benefits increased the amount of health insurance held by low-income households, although these results are less robust. The income-elasticity of health insurance is 0.94, and a \$1,000 increase in benefits increases the number of insurance policies held by the average household 0.08. Finally, increased utilization of health care is concentrated in health care categories that are not covered by Medicare, corroborating the expenditure and health insurance results.

My estimated elasticities are substantially larger than most of the existing literature. Prior work on the effect of income on health care expenditures can be divided into two categories. The first group of studies use aggregate or international level expenditure data to produce national-level income elasticities. The second group use individual or household data with most of them focusing on non-elderly households. Macro estimates using international cross-sections generally produce estimates of income-elasticities near or above one, suggesting that health care is a luxury good at the national level. However, these studies generally rely on simple correlations and lack exogenous variation in income. Many factors that affect both growth in income and health expenditures across countries are omitted in most of these analyses, making the estimates useful for forecasting but unhelpful for identifying the drivers of expenditure growth or understanding individual behavior. For reviews of this literature, see Gerdtham and Jönsson (2000), Getzen (2000), and OECD (2006). Acemoglu et al. (2013) provide a causal estimate of the national income-elasticity of health care expenditures in the United States by instrumenting for income using oil-price shocks. They find a more moderate income-elasticity between 0.7 and 1.1.

Micro estimates of the income elasticities of health care expenditures typi-

cally find elasticities between 0.2 and 0.4 (Newhouse, 1992; Smith et al., 2009), much too small for income to account for a significant share of rising health care expenditures. There are relatively few studies utilizing micro data for several reasons, but most importantly the endogeneity of individual or household income and the lack of data on health expenditures. I am aware of only two studies that address the endogeneity of household income. The most famous, the RAND Health Insurance Experiment (Manning et al., 1987), estimates that the income-elasticity of health expenditures is at most 0.2, far too small to explain a significant share of rising expenditures either per capita or in aggregate. However, the experiment explicitly excluded individuals over 62 years old but, these individuals may have the most elastic consumption of health care.

Tsai (2018) is the only study looking specifically at income and health care expenditures for elderly households. Tsai's empirical strategy is similar to my own. She utilizes the benefits Notch to estimate the responsiveness of health care expenditures in the Consumer Expenditure Survey for 1986-1994. She finds that health care expenditures among the elderly with less than a high school education are highly elastic, with estimates ranging between 1.4 and 3.5. This is in contrast with prior studies that use micro data and generally find small income elasticities, and in fact exceeds the estimated elasticities from many macro studies. Although health care consumption is not sufficiently elastic among younger demographics to drive an increase in expenditures, these estimates suggest rising incomes may play a large roll in increasing per capita expenditures among the elderly on health care. While they do not examine expenditures per se, Moran and Simon (2006) is closely related as they use the Notch to study the effects of income on prescription drug taking among the elderly. They find that the number of prescriptions taken by elderly households is elastic with an elas-

ticity of 1.3 among low-education elderly Social Security recipients.

My findings make several contributions to the literature on the growth of health care expenditures. First, I corroborate Tsai's (2018) finding of highly elastic health care demand among the low-educated elderly in a second dataset and show that these results are robust to a variety of specifications. Second, I estimate the first test for the elasticity of demand for health insurance itself in response to exogenous variation in income and show that the low-income elderly increase the amount of insurance they hold as well as their total premiums paid for health insurance. Prior research has found that the elderly still bear substantial out-of-pocket costs and financial risk due to health expenditures (Goldman and Zissimopoulos, 2003; Marshall et al., 2010). Combined with the large income elasticities for the low-education elderly found by Tsai (2018) and myself, this suggests that despite the availability of public health insurance coverage among this population they remain exposed to substantial out-of-pocket costs for some health care services and highly value additional health insurance. Finally, I examine health care utilization responses of low-education households and show that the patterns of increased health care consumption in response to increases in Social Security income are consistent with the categories of costs generally not covered by Medicare, further corroborating the finding of elastic health care expenditures.

The remainder of this paper is organized as follows. Section 2.2 provides institutional background on the Social Security benefits Notch. Section 2.3 describes the AHEAD/HRS data. Section 2.4 describes my empirical strategy and identification. Section 2.5 presents my empirical results and robustness checks. Section 2.6 concludes.

2.2 Institutional Background: Social Security Benefits Notch

I exploit a natural experiment known as the Social Security benefits Notch, henceforth the Notch, to isolate exogenous variation in Social Security income. Here I provide a brief description of the institutional details for the Social Security formula and policy changes that led to the creation of the Notch drawn primarily from U.S. General Accounting Office (1988). In addition, several existing papers that utilize variation due to the Notch provide further discussion (Krueger and Pischke, 1992; Engelhardt et al., 2005; Snyder and Evans, 2006).

Before 1972 Social Security benefits were determined by nominal average monthly earnings and were not indexed for inflation. Instead, Congress periodically modified the benefits formula to change benefits or adjust for inflation. The relatively high rates of inflation experienced by the US economy in the late 1960s and 1970s led to support for introducing automatic adjustments to Social Security for price changes, as ad hoc adjustments were not sufficiently timely to maintain the purchasing power of benefits. In 1972 Congress amended the Social Security Act to automatically adjust for increases in cost-of-living in two ways. First, benefits were indexed to increase automatically if the annual CPI index rose by 3 percent or more. Second, the maximum taxable earnings ceiling also automatically increased following increases in average covered wages.

However, the change inadvertently led benefits to increase more rapidly than inflation, an error referred to as “double indexation”. The double indexation resulted because average lifetime earnings already increase with inflation, which increases an individual’s future benefit while they are still working. Thus, as the price level rose benefits increased due to increases in the replace-

ment rates in the Social Security formula, but also due to nominal wages rising with inflation which increased the base wage used to compute benefits. As a result, individuals who had yet to retire effectively were compensated twice for inflation. Not only did this flaw in the benefits formula lead to disparities in real Social Security benefits across otherwise similar individuals in different birth cohorts, but the rapid increase in benefits also posed a potential threat to the solvency of the Social Security system.

In 1977 Congress amended the Social Security Act again to provide the intended level of future benefits. However, the new Social Security rules applied to individuals born in 1917 and after. Individuals born prior to 1917 received benefits using the rules from the 1972 amendments and as a result received a permanent increase in Social Security income relative to individuals with similar characteristics aside from being in a different birth cohort. Importantly, the changes in benefits that resulted from the 1972 and 1977 amendments were both large relative to total Social Security income, particularly for individuals with relatively low lifetime incomes, and unanticipated.

In addition to creating variation in Social Security benefits across birth cohorts for otherwise similar individuals, the Notch also creates variation in benefits within cohort and year by retirement age. Individuals who retired before age 65 benefited less from the 1972 amendments than those who retired at 65. Those who retired after 65 benefited substantially more. Thus, the Notch created variation in benefits not only across birth cohorts, but also within cohorts based on retirement age.

2.3 Data

To estimate the effect of Social Security income on health care expenditures I use data from the AHEAD and HRS surveys. The AHEAD survey is a longitudinal panel composed of individuals who were at least 70 years of age in 1993 and their spouses. The sample collected data in two waves, 1993 and 1995, after which AHEAD was integrated with the biennial HRS survey in 1998. The HRS similarly follows a panel of individuals with an initial sample who were at least 50 in 1992. In addition to the waves of the AHEAD survey, I include data from the 1998 and 2000 waves of the HRS. The two studies collect a variety of information regarding income, health care expenditures and insurance, and demographics.

The AHEAD and HRS surveys have a total of 15,231 and 40,942 person observations, respectively, and 11,246 and 27,199 household observations, respectively, for the survey years I use. The household is my primary unit of observation as this is the level at which health care expenditure decisions are likely made, and I assign each household the birth year of its primary Social Security recipient to determine treatment status. Never-married individuals and single men are assigned their own birth year. Married couples are assigned the birth year of the male spouse. The majority of married women in these cohorts received Social Security benefits based on their husband's earnings history (Snyder and Evans, 2006). Widowed and divorced women, however, pose a larger issue as their Social Security payments are likely determined by their former husband's earnings. However, their husband's birth date is not observable in AHEAD/HRS, complicating assignment to treatment and control cohorts. In my baseline estimates I follow prior work and assign widowed or divorced

women to treatment and control groups by subtracting three from their birth year (Moran and Simon, 2006; Cawley et al., 2010; Tsai, 2015, 2018). Three years was found by Engelhardt et al. (2005) to be the median difference between divorced and widowed women and their spouse. However, I additionally consider the subsample excluding previously married women as the Notch is not a strong instrument for them.

I constrain the analysis sample to households where the primary beneficiary was born between 1905 and 1935 and assign households born between 1915-1917 as the treatment group. Moran and Simon (2006), Cawley et al. (2010) and Tsai (2018) all use cohorts born between 1900 and 1930. However, my preferred estimation sample including low-education households and excluding previously married women has fewer than 25 observations per year for birth cohorts before 1905, and thus I instead use the 1905-1935 birth cohorts. Additional observations are excluded for lacking data on Social Security income, health care expenditures, or retirement year. Retirement age is not explicitly reported in the AHEAD/HRS, but many respondents report the year in which they retire. I calculate their retirement age as the difference between the reported year of retirement and their birth year. Finally, I constrain the sample to households with at least \$100 of monthly Social Security income. After these restrictions and dropping observations with missing data my final analysis sample has 16,518 household-year observations on 7,065 unique households. Most households enter the sample in either the 1993 AHEAD wave or 1998 HRS wave and appear in the sample either two or four times, although a smaller number of observations are in the sample one or three times. All monetary variables are converted to 2017 dollars using the CPI-U-RS.

Figure 2.1 shows annual Social Security income for the overall sample, the low-education sample, the low-education sample excluding female widows and divorcees, and the low-education sample of widows and divorcees only. For the overall sample the 1915-1917 birth cohorts show a local peak in Social Security income but no large increase in benefits, particularly compared with later birth cohorts. Among the low-education sample the peak in Social Security income for the 1915-1917 birth cohorts is more pronounced but still does not substantially exceed Social Security benefits for households from birth cohorts in the 1920s. However, for the low-education sample excluding previously married women the benefits Notch is more apparent. The peak benefits in the 1915-1917 birth cohorts are obviously significantly higher than for any other cohort even with no other covariates conditioned out. In contrast, benefits for widows and divorcees are approximately constant across all birth years with perhaps a small local peak in 1915 that declines immediately in 1916 and 1917, which should be the years most benefited by the Notch. This highlights the weakness of the Notch as an instrument for previously married women.

The Notch also created variation in benefits within cohorts by retirement age. Technically Social Security benefits are determined by the age a beneficiary begins drawing them, which may not necessarily be when they retire. However, I only know the date a household began drawing benefits for a small number of households.² Therefore, I assume individuals begin drawing benefits immediately at their retirement date. This seems likely in most cases as there is substantial bunching at age 62, when individuals may first claim Social Security, and at age 65, the “normal” retirement age, as shown in Figure 2.2. In addition,

²Households report the date they began drawing benefits if they began within two years of the survey. By the first survey wave in 1993 the vast majority of respondents had already been receiving benefits for years.

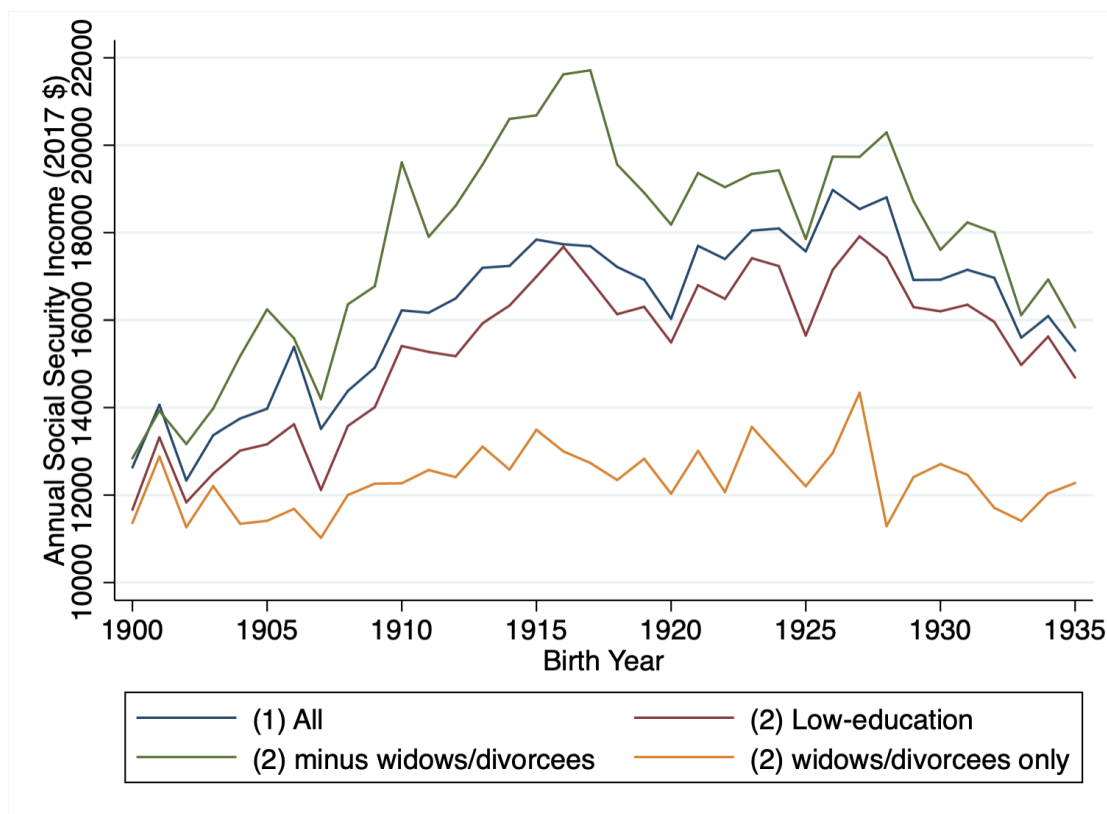


Figure 2.1: Author's calculations using the 1993, 1995, 1998, and 2000 AHEAD/HRS surveys. Each line shows average annual Social Security income for a different sample. The blue line includes all AHEAD/HRS respondents in my sample, the red line excluded those with more than a high school education, the green line excludes previously married women, and the yellow line includes only previously married women.

I assume individuals who retired between ages 55-61 begin receiving Social Security benefits at 62 and exclude individuals who retire before 55 or older than 75. Figure 2.3 shows monthly Social Security income for those who retire before age 65 and those retiring at age 65 or older in the low-education sample excluding widows and divorcees.³ For most years, average Social Security ben-

³Benefits would also be higher for those retiring after age 65 as opposed at 65. However, the AHEAD/HRS surveys do not have the retirement year for all respondents. As a result, my analysis sample is too small to estimate Social Security income in smaller retirement age bins. Appendix Figure B.1 (reprinted from Krueger and Pischke (1992)) shows benefits for workers

efits are higher for those retiring at an older age. The largest differences occur in the 1915-1917 birth cohorts for which older retirees benefited the most from the 1972 amendments.

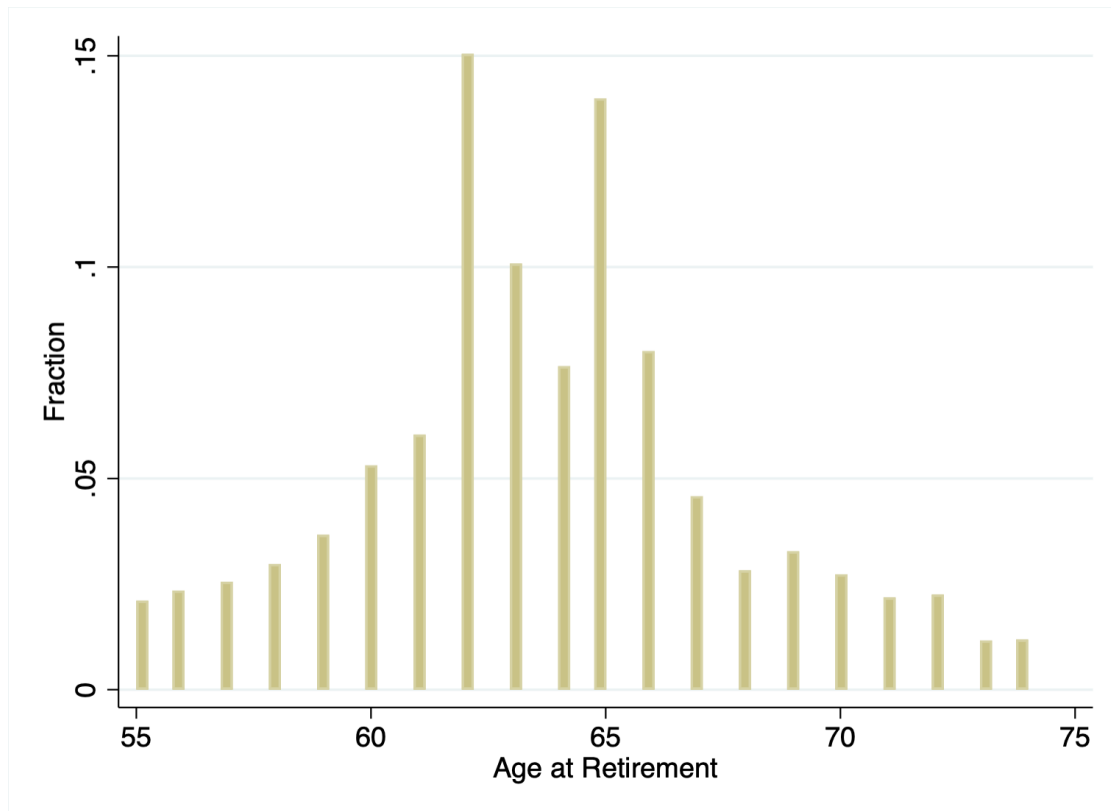


Figure 2.2: Author's calculations using the 1993, 1995, 1998, and 2000 AHEAD/HRS surveys. Each bar shows the fraction of households for which the primary Social Security beneficiary retired at that age. Retirement age is the difference between the reported age of the respondent at retirement and their birth year. Individuals whose calculated retirement age was less than 65 or greater than 75 are dropped.

The questions in the AHEAD and HRS surveys are not consistent across all sample years, particularly concerning health care expenditures. For instance, the 1993 survey uses a one-year recall period instead of the two years used in

with average lifetime wages retiring at ages 62, 65, and 68.

later surveys and groups the various categories of expenditures into two questions rather than four as in later survey years. I convert the responses into an annual overall measure by summing across all expenditure categories and dividing total expenditures in survey years after 1993 in half. In addition, there is a high rate of non-response for many questions, including those pertaining to health care expenditures. I assume that, if a respondent answered at least one question regarding health care expenditures, then any missing answers for other expenditure categories are zero. For more details on differences in survey questions and construction of specific variables see Appendix B.

Table 2.1 presents summary statistics of the analysis sample. Column 1 shows statistics for the entire sample, Columns 2 and 3 show the high- and low-education subsamples, respectively, where low-education is defined as households where the primary beneficiary has a high school education or less. Column 4 shows statistics for the low-education sample with female widows and divorcees removed. Between 11 and 15 percent of the sample are born in the 1915-1917 cohorts. High-education households have substantially higher income, health care expenditures and insurance premiums, and Social Security benefits than low-education households. In addition, high-education households are more likely to be white non-Hispanic and to be married. Similarly, low-education households excluding female widows and divorcees have higher income, Social Security benefits, and health care expenditures than female widows and divorcees, which is unsurprising given that women from these birth cohorts had relatively low labor force participation rates.

While my primary outcome of interest is the total expenditures on health care by a household, I consider additional outcomes including the number of

Table 2.1: Summary Statistics

	(1) All	(2) High Education	(3) Low Education	(4) Excluding Widows and Divorcees
Annual Medical Expenditures (2017 dollars)	1,706.65 (4,642.55)	2,192.21 (5,369.93)	1,499.41 (4,278.27)	1,890.59 (5,085.57)
Annual Medical Expenditures > 0 (2017 dollars)	2,195.69 (5,162.94)	2,560.28 (5,721.57)	2,016.49 (4,855.28)	2,376.26 (5,599.47)
# Insurance Policies	1.68 (0.65)	1.72 (0.68)	1.66 (0.64)	1.66 (0.65)
# Insurance Policies > 0	1.70 (0.62)	1.74 (0.66)	1.69 (0.60)	1.69 (0.62)
Annual Insurance Premiums	1,031.91 (31,223.31)	1,345.76 (31,994.11)	897.95 (30,888.98)	1,195.10 (41,690.51)
Annual Insurance Premiums > 0	2,180.85 (45,365.31)	2,502.53 (43,600.13)	2,015.16 (46,251.90)	2,672.31 (62,316.50)
Monthly SS Income (2017 dollars)	1,414.74 (728.04)	1,589.68 (756.33)	1,340.07 (702.52)	1,582.34 (797.31)
Total Annual Income (2017 dollars)	42,936.93 (125,972.28)	63,839.03 (92,648.87)	34,688.65 (136,034.94)	43,263.86 (183,314.85)
Treatment indicator (born 1915-1917)	0.14 (0.35)	0.11 (0.31)	0.15 (0.36)	0.14 (0.34)
White, non-Hispanic	0.87 (0.34)	0.94 (0.24)	0.84 (0.37)	0.84 (0.37)
Married couple	0.43 (0.50)	0.51 (0.50)	0.40 (0.49)	0.73 (0.44)
Widow/Divorcee	0.42 (0.49)	0.35 (0.48)	0.45 (0.50)	0.00 (0.00)
Never-married	0.04 (0.19)	0.04 (0.20)	0.03 (0.18)	0.06 (0.24)
Age of head	76.18 (6.69)	75.37 (6.59)	76.53 (6.70)	75.27 (6.46)
Less than high school	0.40 (0.49)	0.00 (0.00)	0.57 (0.50)	0.57 (0.50)
High school	0.30 (0.46)	0.00 (0.00)	0.43 (0.50)	0.43 (0.50)
More than high school	0.30 (0.46)	1.00 (0.00)	0.00 (0.00)	0.00 (0.00)
N	16,518	4,637	11,881	6,528

Source: Author's calculations using the 1993, 1995, 1998, and 2000 AHEAD/HRS surveys. Sample is split by education where high education are those households where the primary Social Security beneficiary has more than a high school degree, and low education are those with a high school degree or less. The final column excludes previously married women whose birth cohort is imputed.

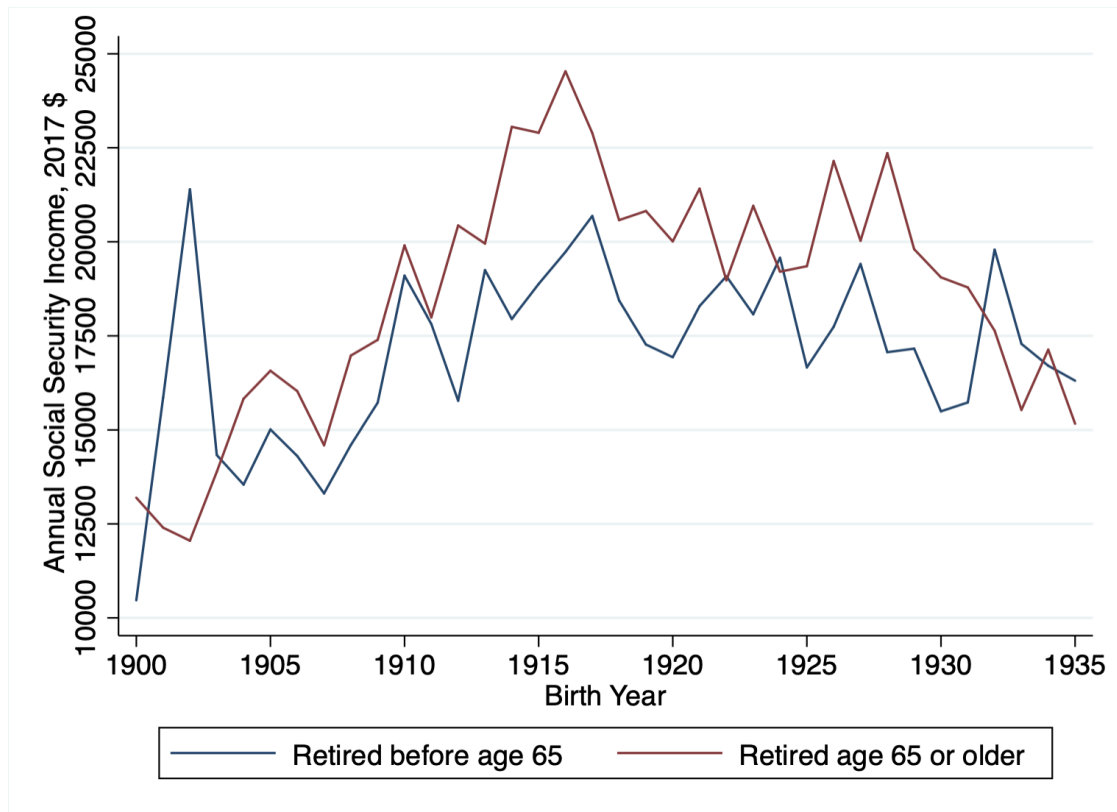


Figure 2.3: Author's calculations using the 1993, 1995, 1998, and 2000 AHEAD/HRS surveys. The blue line shows average annual Social Security income for those retiring before age 65. The red line shows average annual Social Security income for those retiring 65 or older.

health insurance policies held by a household. It may be surprising that there is substantial variation in the number and types of health insurance policies held by the elderly, but Table 2.2 shows the number of households with insurance coverage from various potential sources for the entire sample, the sample of low-education households excluding previously-married women, and within this low-education sample the households in the treatment (Notch) and control groups. Virtually all households report having coverage through Medicare. Medicaid, employer-provided health insurance, and Medigap are also quite

common. Other less frequent sources include CHAMPVA/CHAMPUS which provides coverage for veterans, coverage through unions, self-employment, Railroad Retirement, Mail Handlers insurance, insurance through federal, state, or local government, and a large “other” category. These other sources are not given explicitly by AHEAD/HRS, but could include sources such as the AARP, professional organizations, and state or health alliances. As demonstrated by Table 2.2 there is substantial variation in the amount and sources of health insurance held by households, which suggests insurance may also be a margin through which households may modify their health consumption decisions.

Table 2.2 compares the number of households with different sources because the questions regarding source of coverage are not consistent across survey waves aside from Medicare and Medicaid. CHAMPVA, employer-provided health insurance, Medigap, and “other” are included as categories in every wave although the questions differ somewhat. Other sources are included in only some or one wave, partly explaining the substantially lower frequency. However, my analysis makes use of more comparable questions that are variations on “How many different policies do you have (in addition to Medicare)?” or “How many different employer-provided health insurance plans are you covered by?”. While these questions are not identical across survey waves the AHEAD/HRS inquire about the number of policies in broad categories in every survey wave, and questions about specific sources are follow-up questions to those about the total number. To these totals I add questions about the specific and common sources of health insurance such as Medicare and Medicaid. Thus, the inconsistencies across survey waves about specific sources should not be problematic for my analysis, and the numbers provided in Table 2.2 are only for descriptive purposes. In addition, I included fixed effects for survey wave

Table 2.2: Sources of Insurance Coverage

	(1) All Households	(2) Low Education	(3) Notch Households	(4) Control Households
Medicare	15,586	6,085	779	5,306
Medicaid	1,849	629	62	567
CHAMPVA/CHAMPUS	379	227	29	198
Employer-provided health insurance (EPHI) ^a	3,239	1,340	113	1,227
# of EPHI policies ^b	2,229	971	62	909
Medigap	8,591	3,206	453	2,753
Other	6,797	2,580	429	2,151
Self-employment insurance	45	16	1	15
Union ^b	102	74	11	63
Railroad Retirement ^c	73	28	7	21
Mail Handlers' insurance ^d	20	9	1	8
Federal employees' health insurance ^d	13	1	0	1
Blue Cross/Blue Shield ^d	15	3	2	1
State or local government ^d	7	2	0	2
Other government source ^d	8	1	0	1

Source: Author's calculations using the 1993, 1995, 1998, and 2000 AHEAD/HRS surveys. Each column shows the number of insurance policies reported by households within that sample. Columns 3 and 4 show health insurance coverage for the treatment and control groups within the low education sample.

^a - Covered in 1995, 1998, and 2000 surveys only.

^b - Covered in 1998 and 2000 surveys only.

^c - Covered in 1993 and 1995 surveys only.

^d - Covered in 1995 surveys only.

in my empirical specification which should absorb any differences in insurance coverage that are due to differences in survey questions.

2.4 Methods

Most prior studies of the Notch use empirical strategies that fall into two categories. The first group of papers instrument for the Notch using predicted Social Security income. The predictions of Social Security income are based on the average lifetime earnings of retirees in different demographic cells defined on characteristics such as education and race (Krueger and Pischke, 1992; Engelhardt et al., 2005; Shah Goda et al., 2011; Handerker, 2011; Vere, 2011). The second group of papers instead instrument for Social Security income using a binary indicator for being treated by the Notch. Most commonly these papers assign treatment to the 1915-1917 birth cohorts as these cohorts benefitted most from the Notch (Moran and Simon, 2006; Cawley et al., 2010; Ayyagari and Frisvold, 2016). However, the 1972 amendments that created the Notch applied to the 1910-1916 birth cohorts, while the 1977 amendments applied to cohorts born in 1917 or after. Tsai (2015, 2018) use an alternative indicator for membership in these birth cohorts by instead defining treatment as those born between 1911-1917.⁴

I use the second empirical strategy and assign membership in the 1915-1917 birth cohorts as an instrument for Social Security income. In order to estimate the effect of income on health care expenditures I estimate the following first-stage to predict Social Security income:

⁴The latter strategy is clearly easier to implement. However, Vere (2011) points out that while this strategy is valid the simplicity is a tradeoff with reduced efficiency for exploiting the variation generated by the Notch. A final study falls outside of either strategy. By utilizing a large dataset, Snyder and Evans (2006) are able to estimate a regression discontinuity design on the last quarter of the 2016 birth cohort and the first cohort of the 2017 birth cohort, but the AHEAD/HRS lack sufficient observations to implement such a design.

$$SSI_{ht} = \gamma + \alpha * Notch_h + \phi * X_{ht} + \lambda_a + \kappa_r + \pi_m + \mu_t + u_{ht} \quad (2.1)$$

The subscript h denotes household and t denotes the year the household was surveyed.⁵ $Notch_h$ is an indicator for the households' primary Social Security beneficiary being born between 1915-1917. X_{ht} is a vector of control variables including the race-ethnicity, years of education, and sex of the primary Social Security beneficiary, and indicators for the type of household (married couple, single man, divorced/widowed woman, or never-married woman). In addition I include fixed effects for age, the region in which the household is located, and the month and year in which the household was interviewed.

The second-stage equation is:

$$Y_{ht} = \theta + \beta * SSI_{ht} + \delta * X_{ht} + \lambda_a + \kappa_r + \pi_m + \mu_t + \epsilon_{ht} \quad (2.2)$$

where SSI_{ht} is Social Security income either as reported in the survey data for OLS specifications or predicted by Equation 2.1 for instrumental variable specifications. Y_{ht} is expenditures on health care in my primary specifications, although I also consider related outcomes including health insurance policies, insurance premiums, and health care utilization. Dollar values are converted to 2017 dollars using the CPI-U-RS. All regressions are weighted using the AHEAD/HRS household weights. Standard errors are clustered by cohort year. The coefficient of interest β measures the dollar increase in health care expenditures in response to a one dollar increase in Social Security income as a result of

⁵Within sample waves interviews may take place in different years. For instance, the 1993 and 1995 AHEAD surveys contain observations with interview dates in 1993/1994 and 1995/1996, respectively.

the Notch.

For the Notch to be a valid instrument it must be relevant and exogenous. The Notch is not a strong instrument in all of the samples I consider. It is a weak instrument for households with high education and has an F-statistic only of 14.73 for the overall sample, only slightly above the minimum standard of 10 (Staiger and Stock, 1997). However, it is a much stronger instrument in the subsamples of low-education households with F-statistics of 38.7 and 41.8 when previously married women are included and excluded, respectively.

The exclusion restriction is more difficult given that identification relies primarily on variation in Social Security benefits across birth cohorts. For my empirical model to be identified requires the assumption that the only difference between otherwise similar cohorts that would affect health care expenditures is differences Social Security benefits arising from the 1972 and 1977 amendments. Thus, a primary concern for identification would be if there were policies or events affecting entire cohorts and correlated with the Notch. This creates several difficulties.

First, only three birth cohorts are included in the treatment group which limits the flexibility to exclude specific cohorts to test whether estimates are driven by particular treatment cohorts. Second, the treatment occurs at the birth-cohort level as this is the level at which different Social Security changes and cost-of-living adjustments are made. As a result, standard errors should be clustered by birth cohort, but this requires a tradeoff between including a sufficient number of birth cohorts to avoid having too few clusters and including control cohorts that are increasingly dissimilar from the treatment cohorts. Similar to excluding treatment cohorts, the small number of clusters also exacerbates the issue of

excluding specific control cohorts to test for cohort effects.

Third, Handwerker (2011) mentions several challenges in using the Notch as exogenous variation in income. These include that the Notch could not affect income until retirement, most datasets do not contain information on previously married women's husband's birth year, the Notch did not apply to those on disability insurance or with declining incomes as they neared retirement, and the cross-cohort variation in income was not particularly large. The first two issues are not a problem in my setting. I examine health care expenditures among the elderly specifically when they would be retired. I do not observe the birth year of husbands of previously married women, so I exclude them from my estimation in many specifications.

Handwerker's latter two points are relevant for my context. The first of these points is an issue of instrument strength and Handwerker finds lower reported Social Security benefits in 1916 than in 1915 or 1917, even though 1916 should be a peak year. Even among low-education households a regression of reported benefits on predicted benefits has a t-statistic of only 3.23 which she attributes to insufficient observations. However, Handwerker uses only the 1993 AHEAD survey, whereas I utilize several additional survey waves. While I do not predict Social Security income, I find that the Notch is a strong instrument in my analysis sample, and similarly a weak instrument when using only the 1993 AHEAD wave. In addition, as shown in Figure 2.1, while the Notch is not apparent in the overall sample it is obvious in the sample of low-education households even in the raw data.

Handwerker's second point that the variation in income due to the Notch is small relative to cross-cohort variation in outcomes, in particular labor sup-

ply and mortality, is also relevant to my setting. Variation in income due to the Notch is certainly small in the full analysis sample. Average household income is nearly \$43,000 as shown in Table 2.1, whereas the 1915-1917 birth cohorts are predicted to receive an extra \$930 annually from Social Security, or just over two percent of their total income. However, this includes better educated households that have higher income and benefit less from the Notch. Looking instead at the sample of low-education households excluding previously married women, the 1915-1917 birth cohorts receive around \$2,000 in additional Social Security benefits due to the Notch, or a nearly five percent increase in total income. While this is a relatively small share of their total annual income, it is a permanent increase and increases total Social Security benefits for these households by around 11 percent.

I address these issues in several ways. First, I include an extensive set of control variables in order to account for as much dissimilarity across cohorts that is not driven by Social Security as possible. One benefit of the Notch affecting individuals so late in life is that many characteristics such as an individual's level of educational attainment or labor force status have long since been determined and are unlikely to be correlated with the treatment. In addition, my dataset gives me an advantage over previous studies utilizing this identification strategy that relied on either cross-sectional data or on panel data for which repeated observations occurred within the same year. In both cases these data do not permit the inclusion of age fixed effects which would be collinear with the Notch when it is defined based on year of birth (Moran and Simon, 2006; Cawley et al., 2010; Tsai, 2018). Age fixed effects important because many outcomes, including health care expenditures, are likely related to age. Because my data are a panel over survey waves multiple years apart, I am able to include age fixed effects to

account for older households generally having higher health care expenditures.

Fixed effects for type of household allow for varying marriage patterns across birth cohorts, as well as different time patterns if older households are more likely to be widows or widowers. Region fixed effects account for geographic differences in health care expenditures or costs across nine different regions. Fixed effects for the month and year of the survey allow households to vary their responses due to receiving the survey at different times and thus having different recall periods. In addition, each survey wave administered the survey over one or two years that did not overlap with other survey waves. Thus, survey year effects also control for differences in responses across survey waves due to changing survey questions.

I directly and indirectly test for several potential culprits of cohort effects. A common worry in the Notch literature is the 1918 influenza epidemic. Almond (2006) found that exposure to the flu epidemic while in utero led to worse academic and labor market outcomes, as well as worse health. The 1918-1919 birth cohorts were both potentially in utero during this epidemic but were also both among the first cohorts for which Social Security benefits were determined by the transitional period between the rules under the 1972 and 1977 amendments, during which real benefits fell rapidly. Thus, these cohorts were among the first to face large reductions in Social Security income relative to previous cohorts while simultaneously having higher expected health care costs. This could potentially bias against finding an effect of the Notch as these cohorts would have lower Social Security income but higher expected health care costs. Another concern is that control cohorts born at the beginning or end of the sample period are separated from the treatment cohorts by a decade or more and

may be systematically different from the treatment cohorts. I address these issues by reestimating my empirical model excluding the 1918 and 1919 birth cohorts and then constraining my analysis period to the cohorts born between 1910 and 1925.

The latter restriction in particular raises the issue of having few clusters which can bias standard errors towards zero (Donald and Lang, 2007). Because Social Security is a national-level policy identification relies on cross-cohort comparisons clustered at the cohort level, and prior studies using this identification strategy for the Notch have used the 1900-1930 birth cohorts to ensure a minimally sufficient number of clusters (Moran and Simon, 2006; Cawley et al., 2010; Tsai, 2015, 2018). The restriction to the 1910-1925 cohorts leaves only 16 groups. To address having few clusters Cameron et al. (2008) suggest using critical values from the t_{G-2} distribution which they find works well with fewer clusters.

Some characteristics vary across cohorts that may be related to both Social Security income and health care expenditures, but in ways that simply excluding particular birth cohorts will not control for. Cross-cohort variation in the fraction of men that served in the military or average retirement age are both likely related to health care expenditures. Bedard and Deschenes (2003) show that there is a large increase in the fraction of veterans in each cohort from 1915 until the mid 1920s, and a falling fraction thereafter.⁶ Theoretically the Notch itself could have incentivized differences in retirement behavior across cohorts, although this is unlikely given that the effects of the Notch were unanticipated and relied on high rates of inflation. Furthermore, prior research has found that the Notch did not significantly affect labor supply or retirement behavior

⁶See Appendix Figure B.3.

(Krueger and Pischke, 1992). In spite of this, I test whether veteran status or retirement age are related to my specification of the Notch.⁷

Another concern is that estimates may be an artifact of the specification of the Notch itself. Many papers selected the 1915-1917 birth cohorts because these cohorts had the largest Social Security benefits. However, statutorily the 1972 amendments applied to the 1910-1916 cohorts and the 1977 amendments to the cohorts born thereafter, and Tsai (2015, 2018) for instance use an alternative instrument using the 1911-1917 birth cohorts. I test the sensitivity of my results to this alternative Notch definition. Finally, if the increases in health care expenditures I find are in fact caused by Social Security income from the Notch then there should be a corresponding increase in health care utilization. Moreover, any changes in utilization are unlikely to be uniform across categories of services. As virtually the entire population of my sample has Medicare coverage, services covered by Medicare likely have low out-of-pocket costs and utilization may be less responsive. However, some services such as dental care and prescription drugs are not covered by Medicare.⁸ Because households face higher out-of-pocket costs, utilization of these services should respond more elastically among low-education (and thus low-income) households.

Robustness checks aside, using the Notch as an instrument for Social Security income is a common identification strategy in the literature studying the effects of income for the elderly. It has been invoked repeatedly both in the

⁷For cohort effects it is not always obvious which direction the bias would go. For instance, the 1918 flu epidemic or rising fraction of veterans might suggest that cohorts with less Social Security income had higher health care expenditures, which would bias estimates of the Notch effect downward. However, particularly because I consider a sample of individuals late in life, negative health shocks may lead these cohorts to select for healthier individuals remaining in the sample, which would positively bias the estimated effect of the Notch.

⁸My analysis sample ends in 2000, before the creation of the Medicare Part D prescription drug benefit.

literature using the Notch as a binary instrument as in my setting (Moran and Simon, 2006; Cawley et al., 2010; Shah Goda et al., 2011; Tsai, 2015; Ayyagari and Frisvold, 2016; Tsai, 2018), as well as in the broader literature using predicted Social Security earnings (Krueger and Pischke, 1992; Engelhardt et al., 2005; Handwerker, 2011; Vere, 2011). These studies have also tested the Notch exclusion restriction in a variety of ways and generally failed to produce evidence that it fails.

2.5 Results

2.5.1 The Notch and Health Care Expenditures

Table 2.3 presents my main results for the effects of income on health care expenditures. Each column shows results estimated using a different subsample. Column 1 uses the entire AHEAD/HRS sample, Column 2 uses households with a primary beneficiary that has more than a high school education, and Columns 3-6 use households with a high school education or less, exclude previously married women, and split the sample into households where the head retired before age 65 and age 65 and older, respectively. The first row of Panel A shows the first-stage coefficient from estimating Equation 2.1. The rows below show the percent increase in total income Notch households receive from extra Social Security benefits and the F-statistic for the indicator on Notch membership. A coefficient in the first row is interpreted as the dollar increase in annual Social Security income associated with membership in the 1915-1917 birth cohorts. For the full sample, Notch membership increases household Social Security income

by \$929 per year, which increases total household income by 2.2 percent.

Notch membership has no effect on cohorts with more than a high school education, although the statistically insignificant point estimate is positive. This is unsurprising, as the “double indexation” resulted from the combined effect of both replacement rates and covered wages rising with inflation. However, people with more education tend to have higher wages and are much more likely to have wages above the Social Security maximum. In this case they do not benefit from the indexing of covered wages and would not have significantly different Social Security earnings than other similar birth cohorts. Notch membership is also not a strong instrument in this sample with a F-statistic of just 0.15. For less educated households the Notch has a larger effect, increasing Social Security income by \$1,145, and is a much stronger instrument. The effect is even larger when excluding previously married women who generally have much lower incomes. In addition, as expected the Notch has a larger effect on those who retire at age 65 or later than those retiring before 65. Those who retire at 65 or older gain over \$2,800 in additional annual Social Security income, increasing their total income by seven percent.

Panel B shows results of estimating the effect of Social Security income on health care expenditures from Equation 2.2 using OLS in the first row and the Notch as an instrument for Social Security income in the second row. Regardless of the subsample being analyzed there is little relationship between reported Social Security income and health care expenditures. In fact, most of the OLS estimates are precisely estimated zeros with implied income-elasticities of health care expenditures between 0.2 to 0.4. However, the instrumental variable results indicate a much stronger effect of Social Security income on expenditures.

Table 2.3: The Effect of Social Security Income from the Benefits Notch on Health Care Expenditures

	(1)	(2)	(3)	(4)	(5)	(6)
	Full Sample	High Education	Low Education	Low Education Excluding Previously Married Women	(4) + Primary Beneficiary Retired Before Age 65	(4) + Primary Beneficiary Retired Age 65 or Older
Panel A: First stage						
First-stage coefficient	928.66*** (241.94)	315.77 (814.61)	1,145.67*** (184.28)	2,005.11*** (310.25)	1,747.57*** (495.57)	2,823.00*** (390.60)
% of average total income	2.2	0.5	3.3	4.6	3.7	7.0
F-Statistic	14.73	0.15	38.65	41.77	12.44	52.23
R-squared	0.30	0.28	0.38	0.27	0.35	0.26
Panel B: Second stage						
OLS coefficient on Social Security income (2017 \$s)	0.02 (0.02)	0.03 (0.08)	0.02 (0.03)	0.02 (0.03)	-0.03 (0.06)	0.03 (0.03)
IV coefficient on Social Security income (2017 \$s)	0.11 (0.11)	0.66 (1.47)	0.09 (0.08)	0.20** (0.09)	0.31** (0.14)	0.16* (0.10)
Elasticity	1.10	5.73	0.98	2.56	3.76	2.15
Observations	15,723	4,481	11,242	6,324	3,064	3,260

Source: Author's calculations using the 1993, 1995, 1998, and 2000 AHEAD/HRS surveys. Each column selects a different subsample for analysis. Panel A presents first stage results from estimating Equation 2.1 and Panel B presents both ordinary least squares and instrumental variable results from estimating Equation 2.2. All regressions include the race-ethnicity, years of education, and sex of the household's primary Social Security beneficiary as controls as well as a set of indicators for the type of household (married couple, single man, divorced/widowed woman, or never-married woman), age of the primary beneficiary, region in which the household is located, and the month and year in which the interview took place. Low education is defined as households where the primary beneficiary has a high school education or less and high education are all other households. Column 4 excluded previously married women whose birth cohort is imputed. Columns 5 and 6 split the sample from Column 4 into households for which the primary beneficiary retired before age 65 or at age 65 or older, respectively. Robust standard errors are clustered at the level of the birth cohort. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

The estimates are imprecisely estimated for the whole sample, as well as the high- and low-education samples, but the point estimates suggest an income-elasticity at or above one.

Using the low-education sample excluding previously married women I find that a one-dollar increase in Social Security income increases health care expenditures by 0.2 dollars, significant at the 0.05 level. This implies an elasticity of 2.56, indicating that health care expenditures among this group of the elderly are highly elastic. Splitting the sample by retirement ages shows similar results. Those who retire before age 65 and at age 65 or older have income elasticities of 3.76 and 2.15, respectively. Despite the later retirees benefiting more from the Notch they have a lower income-elasticity of health care expenditures. The difference in elasticities across retirement ages would be unsurprising if there were selection into retirement age. Those who retire at an older age, and particularly those who wait until after the “normal” retirement age of 65, may have systematically better health than those who retire early. Even absent the Notch Social Security benefits are reduced by early retirement, suggesting that those who retire early are penalizing themselves. However, health likely plays a role in retirement, and in fact may play a stronger role than financial decisions (Dwyer and Mitchell, 1999; McGarry, 2004). If those who retire early are in worse health, then it is unsurprising their health expenditures are more elastic than their peer low-educated households who retire later.

My results are broadly consistent with those found by Tsai (2018) and Moran and Simon (2006). Tsai (2018) also examined health expenditure outcomes and found an elasticity of 0.89 for the equivalent full sample, slightly smaller than the elasticity of 1.1 I find for this group. The Notch is similarly a borderline

strong instrument for Tsai's overall sample. She does not report elasticities for the high-education sample but does for the sample of low-education households. She finds an elasticity of health care expenditures of 2.4 for these households overall and 1.91 when she similarly excludes previously married women. My estimate is smaller for the low-education sample overall and larger when excluding previously married women, but both estimate's confidence intervals include Tsai's estimates. Moran and Simon (2006) do not examine expenditures, but look at prescription taking behavior which should be similar if the elderly incur significant out-of-pocket costs for prescriptions. They find a that the number of prescriptions taken by households is also highly elastic, albeit slightly less so than overall expenditures, with a primary estimate of 1.32.⁹

2.5.2 Health Insurance

Out-of-pocket expenditures are highly elastic to additional income among low-education households. However, the high elasticity among low-education households who have much lower income, combined with evidence that the elderly face substantial financial risk despite the availability of public health insurance (Goldman and Zissimopoulos, 2003; Marshall et al., 2010), suggests that there may similarly be demand for additional health insurance coverage. The AHEAD/HRS ask about sources of insurance coverage and premiums paid. Table 2.2 displays summary statistics for the number of households with different sources of insurance. Not all respondents report values for questions related to insurance, further reducing the size of my analysis sample than for health care expenditures.

⁹Neither Tsai (2018) nor Moran and Simon (2006) examine households based on retirement age.

Table 2.4 shows the estimated effects of Social Security income on both the number of insurance policies held and total premiums paid. Panel A repeats the first stage estimation on the subsample for which there is outcome data on insurance coverage and premiums. These estimates are quite similar to those in Table 2.3 with the Notch being the strongest instrument and causing the largest increase in Social Security income among low-education households excluding previously married women, and among those who retired later, while being a weak instrument for high-education households. The magnitude of the benefit from the Notch is similar to the analogous regressions from Table 2.3, although in every case the point estimate is now smaller.

Panel B shows the effects on the number of health insurance policies held by a household. Columns 3 and 4 indicate that the additional income from the Notch cohorts increases the amount of health insurance held by these households. From Column 4, a \$1,000 increase in Social Security income causes households to acquire 0.08 more insurance policies, significant at the 0.05 level. Combined with the estimated effect on income from the first stage, low-education households excluding previously married women have 0.152 more insurance policies as a result of additional Social Security income, with an implied elasticity of 0.94. The effects by retirement age are imprecisely estimated, but positive. In addition, they display the same pattern as for health care expenditures: the point estimate for insurance demand is much more elastic for those who retire before 65 than those who retire 65 or older. Also similar to the results for health care expenditures, the effect is statistically insignificant for the sample overall and for high-education households, but both point estimates are positive.

Table 2.4: The Effect of Social Security Income from the Benefits Notch on Health Insurance

	(1)	(2)	(3)	(4)	(5)	(6)
	Full Sample	High Education	Low Education	Low Education Excluding Previously Married Women	(4) + Primary Beneficiary Retired Before Age 65	(4) + Primary Beneficiary Retired Age 65 or Older
Panel A: First stage						
First-stage coefficient	889.85*** (228.25)	299.96 (812.14)	1,114.40*** (197.00)	1,850.61*** (293.02)	1,660.79*** (486.40)	2,402.43*** (507.73)
F-Statistic	15.20	0.14	32.00	39.89	11.66	22.39
Panel B: Insurance Policies						
OLS coefficient on Social Security income (1000s of 2017 \$s)	0.001 (0.001)	-0.004 (0.005)	0.003* (0.002)	0.004* (0.002)	0.005 (0.004)	0.004** (0.001)
IV coefficient on Social Security income (1000s of 2017 \$s)	0.016 (0.019)	0.238 (2.116)	0.067** (0.028)	0.082** (0.041)	0.163 (0.117)	0.029 (0.023)
Insurance Elasticity	0.16	2.64	0.65	0.94	1.77	0.35
Panel C: Insurance Premiums						
OLS coefficient on Social Security income (2017 \$s)	-0.002 (0.006)	-0.027 (0.032)	0.017 (0.024)	0.0003 (0.003)	0.001 (0.008)	-0.001 (0.002)
IV coefficient on Social Security income (2017 \$s)	0.193 (0.169)	7.243 (53.084)	0.229 (0.164)	0.089 (0.259)	0.104 (0.077)	0.067 (0.345)
Premium Elasticity	0.26	8.56	0.34	0.16	0.17	0.12
Observations	12,237	3,339	8,898	5,019	2,312	2,707

Source: Author's calculations using the 1993, 1995, 1998, and 2000 AHEAD/HRS surveys. Each column selects a different subsample for analysis. Panel A presents first stage results from estimating Equation 2.1, Panel B presents both ordinary least squares and instrumental variable results from estimating Equation 2.2 with the number of health insurance policies held by a household as the dependent variable, and Panel C presents both ordinary least squares and instrumental variable results from estimating Equation 2.2 with expenditures on insurance premiums by a household as the dependent variable. All regressions include the race-ethnicity, years of education, and sex of the household's primary Social Security beneficiary as controls as well as a set of indicators for the type of household (married couple, single man, divorced/widowed woman, or never-married woman), age of the primary beneficiary, region in which the household is located, and the month and year in which the interview took place. Low education is defined as households where the primary beneficiary has a high school education or less and high education are all other households. Column 4 excluded previously married women whose birth cohort is imputed. Columns 5 and 6 split the sample from Column 4 into households for which the primary beneficiary retired before age 65 or at age 65 or older, respectively. Robust standard errors are clustered at the level of the birth cohort. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel C shows the results for premiums paid for health insurance by the household. In no case are the estimates statistically significant, but all point estimates are positive. For low-education households, every subsample predicts that health insurance premiums are inelastic, with elasticities between 0.12 to 0.34. High-education households are the exception, but the Notch is a very weak instrument for these households. Due to the imprecise estimates, however, I cannot rule out that premiums are relatively elastic. For instance, a 95-percent confidence interval for Column 4, the low-education sample excluding previously married women, includes an elasticity of up to 1.07. Due to sample restrictions and non-responses to some survey questions, note that for both health insurance outcomes the sample sizes for estimates by retirement age rely on particularly small analysis samples with only 200-300 treated observations and around 30 observations in many control birth cohorts.¹⁰ However, the estimated effects for premiums paid out-of-pocket are consistent with an increase in insurance coverage, as well as an increase in health care expenditures.

These results corroborate the evidence on health care expenditures that the low-income elderly have elastic demand for additional health care and consume additional health care along multiple dimensions. Moreover, while Tsai (2018) also studied out-of-pocket expenditures among the elderly, so far as I know these are the first results studying the behavior of the elderly towards health insurance and the first to comprehensively study the different margins on which demand for health care could respond. These findings are consistent with the literature that finds that the elderly have high out-of-pocket costs for health care despite public health insurance and are still exposed to high levels of financial

¹⁰The analysis sample is not uniformly distributed across cohorts. The 1913-1923 cohorts generally have the most observations with far fewer for the cohorts at either end of the analysis period, but especially cohorts born before 1910.

risk (Goldman and Zissimopoulos, 2003; Marshall et al., 2010). Moreover, my results contribute to the literature by demonstrating the heterogeneity of health and financial risk among the elderly. This suggests that policy interventions aiming to improve elderly welfare and assist those most exposed to financial risks could focus in particular on the elderly who retire early, as these individuals have worse health and more elastic demand for additional health care. In the next section, I also provide evidence that the early-retirees not only generally demand more health care but do so in specific categories of services that tend to have less or no coverage from publicly provided health insurance.

2.5.3 Robustness

I perform a variety of robustness checks to corroborate my results. Table 2.5 presents these checks for four specifications: health care expenditures and the number of insurance policies outcomes for both the low-education and excluding previously married women subsamples.¹¹ One concern is that the birth cohorts from the early 1900s, late 1920s, and early 1930s may not be good controls for cohorts born between 1915-1917. Real average lifetime incomes rise for these cohorts, meaning that those born near 1900 are poorer throughout their lives than the treatment cohorts, while those born nearer 1930 are richer. Similarly, there is a trend towards increasing educational attainment. In Column 1 of Table 2.5 I restrict the analysis sample to the birth cohorts born between 1910-1925 to limit the comparison to more similar birth cohorts. The estimated coefficients are similar in direction and magnitude to those estimated in Table 2.3. However, these estimates are not statistically significant even when using conventional

¹¹I do not include robustness checks for specifications using retirement age due to small sample sizes.

thresholds, much less when using the modified, larger critical values proposed by Cameron et al. (2008) to account for the small number of clusters.

Table 2.5: The Effect of Social Security Income Excluding Specific Birth Cohorts and Using an Alternative Instrument Definition

	(1) Sample Restricted to 1910-1925 Birth Cohorts	(2) Alternative Notch Using 1911-1917 Birth Cohorts	(3) Excluding the 1918-1919 Flu Birth Cohorts
Health expenditures, low-education sample (2017 \$s)	0.052 (0.08)	0.051 (0.06)	0.078 (0.08)
Health expenditures, low-education excluding widows and divorcees (2017 \$s)	0.21 (0.16)	0.14** (0.06)	0.16** (0.07)
Insurance, low-education sample (1000s of 2017 \$s)	-0.011 (0.021)	-0.012 (0.019)	0.093 (0.012)
Insurance, low-education excluding widows and divorcees (1000s of 2017 \$s)	0.030 (0.023)	0.034 (0.021)	0.064 (0.040)

Source: Author's calculations using the 1993, 1995, 1998, and 2000 AHEAD/HRS surveys. Each column modifies the sample or instrument of my base specification. Each cell is the coefficient on predicted Social Security income from an IV estimate of Equation 2.2. Rows differ by the outcome considered (health care expenditures or number of insurance policies) and which subsample was used (either low-education households overall or those households excluding previously married women). All regressions include the race-ethnicity, years of education, and sex of the household's primary Social Security beneficiary as controls as well as a set of indicators for the type of household (married couple, single man, divorced/widowed woman, or never-married woman), age of the primary beneficiary, region in which the household is located, and the month and year in which the interview took place. Low education is defined as households where the primary beneficiary has a high school education or less and high education are all other households. Robust standard errors are clustered at the level of the birth cohort. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

It is unclear whether the loss of significance results from the removal of bias from using less similar control cohorts or from the reduction in sample size. The largest subsample is Row 1 with 8,317 observations, while Row 4 has only 4,017. The point estimates remain consistent with those from Tsai (2018). The Notch does create within birth cohort variation by retirement age, with those who retire at an older age benefitting more from the Notch. With a sufficiently larger dataset, one theoretical solution to the tradeoff between a sufficient number of clusters and a wide range of birth cohorts would be clustering by the birth cohort and retirement year. However, due to non-responses on the question regarding retirement year the AHEAD/HRS survey does not have a sample of sufficient size to use within-cohort clusters while also further restricting the

number of birth cohorts.¹²

Rather than defining Notch membership as the 1915-1917 birth cohorts, Tsai (2015, 2018) uses the 1911-1917 birth cohorts. Column 2 uses this alternative Notch definition and repeats the estimation of Equation 2.2 for the 1905-1935 birth cohorts. The effects do not depend on which Notch definition is used. The estimated coefficient for Social Security income with health care expenditures as the dependent variable and using the low-education sample excluding previously married women is similar at 0.14 versus 0.20 when the 1915-1917 cohorts are used and remains significant at the 0.05 level. The effect on health insurance is no longer significant and only about half the size (0.034 versus 0.082) but remains positive.

Cohort effects are a major threat to identification and I next examine some potential culprits. Column 3 of Table 2.3 re-estimates Equation 2.2 excluding the 1918-1919 birth cohorts that were affected by the influenza epidemic. The estimated coefficients are mostly unchanged although the effect on health insurance loses significance, suggesting that the 1918 flu is not driving my results. This is consistent with the findings of prior work that has not found excluding the flu-affected cohorts to be important (Moran and Simon, 2006; Tsai, 2018). Other potential cohort effects would be varying rates of military service or retirement ages across cohorts. In Table 2.6 Columns 1 and 2, I regress these outcomes on an indicator for Notch membership. Panel A shows the effect for the low-education sample and Panel B for the same sample excluding previously married women. The estimate in Panel B for veterans suggests that there may be a marginally statistically significant negative relationship between Notch membership and

¹²Another solution, albeit substantially more complex, would be using predicted Social Security earnings which are a much stronger instrument as shown by Vere (2011).

veteran status.

This is unsurprising as the fraction of men who are veterans rose rapidly between 1915 and the mid-1920s (Bedard and Deschenes, 2003). Controlling for veteran status and retirement age is less straightforward than outrightly excluding other candidates for cohort effects, however, because every cohort has some veterans and variation in retirement age. Simply excluding veterans would be problematic if there were selection into who is a veteran. For instance, if veterans tended to be drawn from the more physically fit who would otherwise have lower life-time expected health care costs, this would affect health care spending across birth cohorts in a way that is potentially related to health expenditures. In addition, veteran status may directly interact with out-of-pocket expenditures if veterans are covered by additional health insurance or health care through the Department of Veterans Affairs. In Column 2 I regress retirement age on Notch membership. In both samples the estimated coefficient is negative and indicate the Notch cohorts retired between 0.2 and 0.3 younger but are not statistically significant. This is unsurprising given the findings of Krueger and Pischke (1992) that income from the Notch did not affect labor supply trends.¹³

Finally, the positive estimates on health care expenditures indicate that uti-

¹³Despite the potential endogeneity I have attempted to estimate regressions using veteran status and retirement age as controls. However, both are problematic. Veteran status has a high non-response rate in my analysis sample which cuts the number of men in my sample from 8,901 to 4,263. Moreover, sample size aside the reported proportion veteran have only roughly the correct trend when compared to Bedard and Deschenes (2003). Appendix Figures B.2 and B.3 graph the fraction of veterans in my sample and estimated by Bedard and Deschenes (2003). The fraction of veterans in my sample is approximately 20 percentage points too low in 1915, rises abruptly between 1923 and 1925, and then maintains an approximately constant level around 10-15 percentage points too high for the remainder of the birth cohorts. Bedard and Deschenes (2003) find a rapid but constant rise between 1915 and 1920, a brief plateau until around 1927, and then a staggered decrease in the fraction veteran. Including retirement age as a control is problematic because, while there are many observations overall in the retirement age bins when dividing the sample by those who retired before and after 65, this is not true for individual age bins. Many individual age bins are collinear with the combination of the indicator for the Notch, education level, and marital status/household type.

Table 2.6: Alternative Specifications and the Effect of Social Security Income on Health Care Utilization

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Veteran Status	Age at Retirement	log Income and Expenditures	Any Utilization	Visited Hospital	Nursing Home Stay	Visited Doctor	Had Surgery	Dental Work	Had Any Prescriptions	Number of Prescriptions
Panel A: low-education households											
Notch cohort indicator	-0.117 (0.073)	-0.380 (0.317)									
Log expenditures and income			0.834 (1.280)								
Social Security income (1000s of 2017 \$s) and utilization				0.051 (0.073)	-0.194 (0.234)	0.115* (0.065)	0.297 (0.190)	-0.127 (0.175)	0.366** (0.159)	0.070 (0.147)	0.103 (0.097)
Panel B: low-education households excluding previously married women											
Notch cohort indicator	-0.313* (0.177)	-0.167 (0.417)									
Log expenditures and income			1.169* (0.610)								
Social Security income (1000s of 2017 \$s) and utilization				0.036 (0.046)	-0.065 (0.144)	0.095*** (0.034)	-0.202* (0.108)	0.172 (0.120)	0.161* (0.097)	0.330*** (0.073)	0.845 (0.943)

Source: Author's calculations using the 1993, 1995, 1998, and 2000 AHEAD/HRS surveys. Each column considers a different outcome and each cell is the coefficient from a different regression. Panel A uses all low-education households while Panel B uses those without previously married women. Within each panel the first row estimates Equation 2.1 with a different dependent variable, the second row estimates Equation 2.2 using IV and the natural logarithm of the dependent variable and predicted Social Security income, and the third row estimates Equation 2.2 using IV with health care utilization measures as the dependent variable. All regressions include the race-ethnicity, years of education, and sex of the household's primary Social Security beneficiary as controls as well as a set of indicators for the type of household (married couple, single man, divorced/widowed woman, or never-married woman), age of the primary beneficiary, region in which the household is located, and the month and year in which the interview took place. The last column is an exception because the number of prescriptions was only reported in the 1993 AHEAD survey, and thus this column excludes age fixed effects which would be collinear with the Notch defined by birth year. Low education is defined as households where the primary beneficiary has a high school education or less and high education are all other households. Robust standard errors are clustered at the level of the birth cohort. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

lization of health care services increases in response to income. In Columns 4-11 I estimate the effect of Social Security income on a variety of measures of utilization. Column 4 is the effect on the probability of any utilization, Columns 5-10 are the change in probability for specific services in the last 12 months, and Column 11 is the number of prescriptions the household reports taking per month.¹⁴ Many of these estimates are statistically insignificant, and in fact the estimated effect of income on the probability of seeing a doctor in the last 12 months is marginally significant and negative. However, there are statistically significant, positive estimates for the probability of staying in a nursing home, getting dental services, and taking any prescriptions. These results are reassuring because these are the elective care services that many elderly may not have insurance coverage for and face higher out-of-pocket costs. Medicare does not typically cover dental services or prescription drugs. Nursing home stays are covered under Medicare Part A, but with more limitations than other services such as hospital visits.¹⁵ The fact that increases in utilization are concentrated in services for which the elderly likely have the highest out-of-pocket costs corroborates the effects found for health care expenditures overall.

2.6 Discussion

In this paper, I exploited changes to Social Security rules that conferred a large, unanticipated increase in benefits to particular birth cohorts to estimate how variation in income affects expenditures on health care. In addition, I provide

¹⁴The number of prescriptions taken is only reported in the 1993 AHEAD wave.

¹⁵For more information on services covered by Medicare, see: <https://www.cms.gov/Outreach-and-Education/Medicare-Learning-Network-MLN/MLNProducts/Downloads/Items-Services-Not-Covered-Under-Medicare-Text-Only.pdf>

the first-ever estimates of the effect of income on health insurance coverage among the elderly. I find that not only are health care expenditures highly elastic for elderly households with less than a high school education, with elasticity estimates ranging from 0.98 to 3.76, but also that these households increase the number of insurance policies that they hold and have a consistent positive, although statistically insignificant, increase in insurance premiums. These results are corroborated by an increase in health care utilization in categories of services for which the elderly would be expected to bear large out-of-pocket costs despite the widespread availability of publicly subsidized health insurance, particularly dental services and prescription drugs.

These findings are narrow in the sense that they do not help resolve the broader question of the extent to which rising incomes have driven the increase in health care expenditures as a share of GDP. However, they do demonstrate that, at least for some subgroups of the population, health care expenditures can be highly elastic. There is insufficient publicly available data to estimate per-enrollee Medicare expenditures for the population with less than a high school education. However, as a back of the envelope calculation, between 1975 and 2015 total Medicare expenditures per enrollee increased by nearly 330 percent from just over \$2,600 per enrollee to nearly \$11,200.¹⁶ Over the same period median household income (including net taxes and cash transfers) for elderly households with a high school education or less increased nearly 70 percent.¹⁷ Combined with the preferred elasticity estimate of 2.56 for low-education

¹⁶Author's calculations using expenditure data from the National Health Expenditure Account Tables. Note that Medicare expenditures include administrative expenditures as well, not just spending on enrollees, and thus this is the total program cost per enrollee. 1975 dollars adjusted to 2015 using the CPI.

¹⁷Author's calculations using the March Current Population Survey. I adjusted median incomes using the CPI. Median income of these households was \$27,259 in 1975 and \$45,632 in 2015.

households excluding previously married women and assuming the 330 percent increase in expenditures per enrollee was constant across enrollee characteristics, rising incomes would increase health care expenditures among this group by 170 percent, explaining just over half of the total increase. While household-level income-elasticities are too small to explain the rising expenditures in the general population, at least in low-education households rising incomes could play a major role.

If rising incomes are driving the increase in expenditures among low-education households this suggests that rising expenditures are welfare increasing. However, the pattern of utilization in response to an income shock indicates that there may still be justification for a policy response. In particular, low-income elderly households that likely have poor health appear to spend a significant amount of their additional Social Security benefits on nursing homes, dental services, and prescription drugs. As Medicare Part D now covers prescription drugs policy makers considering expansions to Medicare services could focus on the prior two categories of services that retain high levels of out-of-pocket costs. Conversely, Medicare cuts may place significant additional financial burden on low-income elderly households.

CHAPTER 3

**INCOME GROWTH AND ITS DISTRIBUTION FROM EISENHOWER TO
OBAMA: THE GROWING IMPORTANCE OF IN-KIND TRANSFERS
INCLUDING MEDICAID AND MEDICARE (1959-2012)**

3.1 Introduction

President Eisenhower held the first White House Conference on Aging in January 1961, at which health insurance for Social Security beneficiaries was proposed. Five years later, a central feature of President Johnson's Great Society legislation, the Social Security Act of 1965, launched Medicare and Medicaid.¹ Expenditures on these two programs—see Figure 1—grew from zero in 1959 to \$5.9 billion in 1966 to \$728.5 billion in 2012 (in 2012 dollars), exceeding the combined 2012 expenditures on Old-Age, Survivors, and Disability Insurance (OASDI) and Supplemental Security Income (SSI). Yet traditional measures of the importance of government taxes and transfers on the after-tax income of Americans do not include a value for in-kind transfers such as Medicare and Medicaid.

The U.S. Congressional Budget Office (CBO) (Congressional Budget Office 2013), in 2012, was the first government agency to include the market value of both government- and employer-provided health insurance in their comprehensive measure of income.² Most recently Larrimore, Burkhauser, and Ar-

¹See Andersen, Lion and Anderson (1976) and Moon (1993) for early histories of Medicare, and Blumenthal, Davis, and Guterman (2015) for the most recent overview of this program. See Gruber (2003) for a history of Medicaid.

²A small academic literature has begun to include the market value of health insurance in its measures of income. See Burtless and Svaton, 2010; Burkhauser, Larrimore and Simon, 2013; Burtless and Milusheva, 2013; CBO, 2013; Sommers and Oellerich, 2013; and Armour,

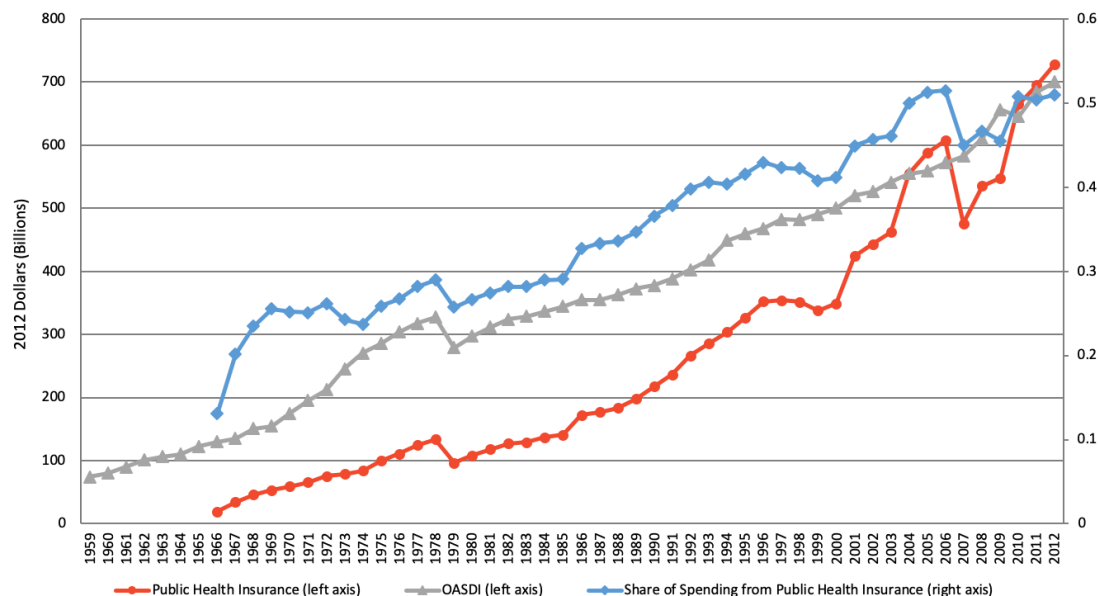


Figure 3.1: Yearly Aggregate Spending on Medicare/Medicaid and OASDI/SSI (in 2012 dollars). *Sources:* Authors' calculations using March CPS, NHEA (CMS 2018), SSA 2011, and Social Security Administration Trust Fund Data (2014). *Notes:* Public health insurance is the estimated total expenditures of Medicare and Medicaid. These are calculated from CPS-reported market values post-1979, and using administrative data pre-1979. OASDI is the sum of total expenditures of the Old-Age, Survivors, and Disability Insurance and Supplemental Security Income programs as reported by the Social Security Administration.

mour (2015) used this same fuller measure of income for the period 1979-2012 to demonstrate the effects of these resources on trends of income and its distribution. Here we use it to measure after-tax income (including the market value of Medicare and Medicaid and other in-kind transfers) and its distribution across American households further back to 1959—just before the major expansions of government tax and transfer programs associated with the New Frontier and

Burkhauser, and Larrimore, 2014). But because this literature has been dependent on U.S. Census Bureau measures of the market value of health insurance, its analyses only go back to 1979.

Great Society programs of the 1960s.³

Although the March Current Population Survey (CPS) has been conducted annually since income year 1967, the U.S. Census Bureau has only estimated the market value of Medicare and Medicaid and linked these values to the CPS data since 1979. Furthermore, the CPS contains little information on government in-kind transfers more generally before then, a period well after the start of the New Frontier and Great Society programs of the 1960s.

To overcome these data limitations, we use March CPS data (income years 1967-2012) and decennial Census data (decennial income years 1959-1989) to create common yearly source of income categories, including estimates of the market value of in-kind transfers, back to 1959. Hence our first contribution to the literature is related to data development. We extend and make available for public use a market value of Medicare/Medicaid series for the CPS from 1967 through 1978 that is consistent with the values produced by the Census Bureau thereafter. We do the same for the Census Bureau market values of employer-provided health insurance, although data limitations make our employer-provided health insurance series less exact. Our paper is the first to estimate the value of Medicare, Medicaid, and employer-provided health insurance and include them in measures of income for the years prior to 1979. We also include estimates for food stamps, housing subsidies, and the school lunch program, although we are not the first to estimate these transfers (Fox et al.,

³Fox et al. (2015) estimate poverty rates in the United States back to 1967 using income concepts from the Supplemental Poverty Measure (SPM). Hence they also subtract taxes from gross income and include the market value of some in-kind transfers as resources and in their threshold measures. However, the SPM ignores the market value of government- and employer-provided health insurance in its measures of household resources and thresholds. Although the SPM provides a consistent relationship between the resources counted as income and included in the poverty threshold, it will fail to capture the growing importance of Medicare and Medicaid to Americans. Instead of treating the market value of health insurance as a resource, it instead subtracts medical out-of-pocket expenses from total household resources.

2015)

For our second contribution we create analogous series using the decennial Census for the years 1960, 1970, 1980, and 1990 (income years 1959, 1969, 1979, and 1989), and show that they yield similar values to those found using CPS data for those years. This allows us to couple our decennial Census-based 1959 values to our CPS values from 1967-2012 to create common yearly source of income categories including estimates of the market value of in-kind transfers, back to decennial Census income year 1959. Reassuringly, we find that our CPS and decennial Census year income levels and distributions are similar for common years. Third, we show how iteratively more comprehensive measures of income and sharing units, up to including the market value of Medicare and Medicaid, contribute to income trends and the evolution of the income distribution back to the initial creation of the major Great Society programs.

Substantively, we use these data to provide a fresh look at the twenty-year period 1959 (the last business cycle peak year of the 1950s) to 1979 (the business cycle peak year starting point of the modern survey-based literature on trends in U. S. income and income inequality) that encompasses the start of New Frontier and Great Society programs. We establish that after increasing substantially from 1959 to 1969, median market income (wages, interest, dividends, rents, etc.) fell from 1969 to 1979 and market income inequality increased. But we also show that concurrent increases in government taxes and transfers when more fully measured to include the market value of Medicare and Medicaid and other in-kind transfers resulted in increases in median income over the entire period and a decline in income inequality.

Using all the years of our data from business cycle peak year 1959 to busi-

ness cycle peak year 2007—and for completeness from 1959-2012—we show that when fully measured, growth in government taxes and transfers has offset the substantial decline in the growth of market income for those in the bottom half of the income distribution since business cycle peak year 1969. We conclude that conventional measures of median income and income inequality that exclude the market value of in-kind transfers including Medicare and Medicaid will substantially understate the success of government policies in offsetting the stagnation of median market income growth and the rise in market income inequality since 1969.⁴

3.2 Data and Methods

Drawing on previous work, we use the public-use March CPS data to construct estimates of household income building on income series from Armour et al. (2014), and supplemented with cell-means from Larrimore et al. (2008), to address top-coding of high sources of income in households. With these data, we extend the CPS household income series created in Larrimore et al. (2015) back to 1959—the last business cycle peak year before major increases in government transfers related to both the maturing of Social Security (OASDI) and the launch of Great Society programs in the 1960s. Most especially, we capture, for the first time, the importance of in-kind transfers including the market income of Medicare and Medicaid on measures of income and its distribution over this period.

⁴We define a business cycle peak year as the peak in our median market income of tax unit series since it is capturing market income. These years usually correspond to the last full year of macroeconomic growth as defined by the NBER and identified in our figures but are the second to last full year of macroeconomic growth before the recessions of early 1990s and 2000s. This measure is similar to that used by Armour, Burkhauser, and Larrimore (2014) and Daly and Valletta (2006). Our findings are not sensitive to using the last full year before a recession in all cases.

As with previous work, we address the known break in CPS data between years 1992 and 1993 resulting from a change in Census Bureau data-collection methods using the same method: upwardly adjusting series for earlier years to generate a complete series with no change between 1992 and 1993 (see Atkinson, Piketty, and Saez, 2011; Burkhauser et al., 2012a; and Armour et al., 2014).

The modern CPS series begins in 1968 (income year 1967). We use these data to estimate market income of tax units back to 1967, utilizing methods that are consistent with those in the tax-record-based inequality literature as well as three alternative estimates of income using methods that are consistent with those in the household-survey-based literature.⁵ Because many major Great Society programs began before 1967, most especially Medicare and Medicaid, this is not an ideal year to begin a study of the importance of government taxes and transfers on household income. Furthermore, to separate trends in income growth from variations introduced by business cycles, previous studies have compared peak years in the business cycle (Burkhauser et al., 2012a; DeNavas-Walt et al., 2013; Armour et al., 2014). Since 1967 is not a peak year in the business cycle, the earliest year in a series beginning in 1967 that we can consistently compare with subsequent peak years is 1969.

For these reasons we create a second set of income series using the decennial Census of 1960. This corresponds to income year 1959, which is a peak year in the business cycle. Thus, we can make comparisons between peak years 1959 and 1969 and thereby capture the importance of Medicare, Medicaid, and other in-kind transfers during the 1960s. To establish that the Census-based data

⁵We extend our CPS series back to 1967 rather than to 1965 or earlier even though CPS data does exist for some of these years. We do so because sample sizes are smaller and because income questions in these years are considerably less detailed. This makes it more difficult to establish income categories consistent with those beginning in 1967.

points in 1959 can reasonably be combined with those of our CPS income series, we repeat the process for the 1970, 1980, and 1990 decennial Censuses that can be directly compared to data for the same year in the CPS. Below we briefly describe these four alternative measures of income. We more fully discuss the details of our sources of income imputations in these series in the Appendix.⁶

3.2.1 Market Income of Tax Units

A major new international literature based on data from administrative tax records of rich countries traces the share of income held by the very top part of the income distribution of these countries back to the early part of the 20th century. But, for the United States, this literature's measure of income is limited to taxable market income (wages, interest, dividends, etc.) of tax units. See Atkinson et al. (2011) for a review of this international literature and Piketty and Saez (2003) for the first effort to measure top income shares in this way for the United States.

We follow Piketty and Saez (2003) and define market income to include gross income from wages and salaries, farm income, self-employment and business income, retirement income from pensions, dividends, interest, rent, and alimony. These sources of income are summed across individuals in a tax unit within each CPS household, without adjusting for number of persons in a tax unit. Our unit of analysis, therefore, is the tax unit.

⁶We also develop labor earnings of tax unit series. We use this measure as an additional check on the comparability of our decennial Census and CPS series. We do this because the decennial Census and CPS ask similar questions with respect to wage earnings of tax units. This is not the case with respect to the market income of tax units, necessitating some imputation (see the Appendix for details). Wage earnings include: income from wages and salaries, farm income, and self-employment and business income.

While some of these separate sources of income are combined in earlier CPS years, each is included in some CPS question back to 1967. Some of these sources of income are not specifically included as decennial Census questions. In particular, in earlier years, retirement and pension income, dividends, interest, rent, and alimony are grouped as “other” income, a category that also includes some non-market sources of income such as OASDI. Some of these sources are covered separately in later years while other sources continue to be grouped as “other” income. As a result, imputation of these sources varies, both across decennial Census and CPS surveys and over time within the decennial Census. Tax units are not explicitly defined in the CPS or the decennial Census, and so we assign tax units using the same assumptions from Piketty and Saez (2003). Single individuals over 20, married couples, and divorced or widowed individuals are assigned to separate tax units. Never-married children under 20 are assigned as dependents to their parent, guardian, or a households primary family. See the Appendix for details.

3.2.2 Household Size-Adjusted Pre-Tax Post-Transfer Income of Persons

Household size-adjusted pre-tax post-transfer income of persons expands the sharing unit from the tax unit to the household and adds in-cash social insurance and welfare income to the market income definition, but it does not include income from tax-credits, in-kind transfers, or the value of health insurance. We include OASDI, SSI, unemployment insurance/workers compensation, veterans’ payments, and cash payments from programs such as AFDC/TANF. The

Census Bureau in their annual report on income has used these sources of income since the 1970s. (See: Fontenot et al., 2018, for the most recent version of this report.) Consistent with the survey-based literature, our unit of analysis is the person. We adjust this measure's household income using the square root of the number of people in the household and assume equal sharing across household members. This size adjustment is common in U.S. and international research studies of median income trends and inequality (for example, see Ruggles, 1990; Gottschalk and Smeeding, 1997; Atkinson and Brandolini, 2001; d'Ercole and Förster, 2012).

As with market income, the income categories covering these income sources are less granular in earlier CPS surveys. However, while aggregated, the various categories are still covered by some questions in the CPS back to 1967. This is also the case with respect to the decennial Census. Therefore, unlike our measure of market income, it is not necessary for us to impute any decennial Census income sources to align them with the CPS for this measure of income. The reason is that while the different income categories are grouped by survey questions, the groups all align with the income sources included as pre-tax post-transfer income. For example, retirement, investment, and public assistance income are all grouped under a single question in the 1960 decennial Census. This mixes market sources of income with government transfers, but all three sources of income are included in a pre-tax post-transfer measure of income. This is likely our most comparable income series since it requires no income source or tax unit imputations.

3.2.3 Household Size-Adjusted Post-Tax Post-Transfer plus In-Kind Transfer Income of Persons

Post-tax post-transfer plus in-kind transfer income includes changes in income due to the tax system, and three sources of in-kind transfers: food stamps (SNAP since 1996), school lunches, and housing subsidies. We calculate tax credits and liabilities back to 1977 using NBER Taxsim 9.3. Estimates of the market value of these major government in-kind transfers programs—food stamps, school lunches, and housing subsidies—going to households are provided by the Census Bureau in the CPS beginning in 1979. For food stamps, school lunches, and housing subsidies we follow Fox et al. (2015) to impute receipt and benefit amounts using the predicted probability of receipt and administrative data on the total number of recipients and program expenditures. We discuss the details of how we extend these tax and in-kind series back to 1967 in the Appendix. We are not able to include a market value for these in-kind transfers in our decennial Census series, because the Census Bureau provides neither estimates of receipt nor the value of in-kind transfers for its decennial Census.

3.2.4 Household Size-Adjusted Post-Tax Post-Transfer plus In-Kind Transfer Income plus Health Insurance of Persons

In constructing this fourth measure, we note that in-kind benefits in the form of health insurance, like all other in-kind benefits, have value to individuals—otherwise employees would negotiate higher wages in exchange for foregoing

health insurance and government actors would have a strong incentive to replace Medicaid and Medicare benefits with cash transfer programs or lower taxes. Measures that exclude the value of health insurance as a resource under-value its worth by effectively placing a zero value on access to this resource. This exclusion understates not only the level of household resources but also their trend, as the cost of health insurance purchased in the marketplace (its market value) has increased in both absolute terms and as a share of wage compensation; it has also increased as a share of all government transfers to households.

Following the approach of Burkhauser, Larrimore, and Simon (2012b) and the CBO (2012, 2013), we include the market value of health insurance in this measure of income back to 1979 based on the Census Bureau's imputed value of health insurance, although we use the full market value rather than just its fungible value. The Census Bureau imputes the value of employer-sponsored health insurance by first determining whether the individual is covered by an employer-sponsored plan and whether the employer paid for all, part, or none of the plan premium. Next, persons in the March CPS are statistically matched to persons in the National Medical Care Expenditure Survey or Medical Expenditure Panel Survey (depending on survey year) based on several demographic characteristics to impute the cash value of employer contributions. The Census Bureau uses this imputed value as its measure of the market value of employer-provided health insurance for covered workers. Individual expenditures on employer-sponsored health insurance plan premiums or expenditures on small-group/individual market health plans come from other income sources and are not included as income.

For government-subsidized health insurance (Medicare and Medicaid), the Census Bureau determines, by state and risk class back to 1979, the average government cost of providing Medicare and Medicaid to those persons reporting that they have this insurance. The two risk classes for Medicare are aged and disabled. The four risk classes for Medicaid are aged, blind and disabled, nondisabled children (less than 21), and nondisabled adults (21-64).⁷ Thus, the imputed average cost of government-provided health insurance varies by state and by the government insurance pool from which it is accessed by beneficiaries.

In determining the value of Medicaid and Medicare, for individuals who qualify for both programs (dual eligible), we follow the Census Bureau's approach and estimate the value of their health insurance as the combined cost of insurance from each program. CBO (2012), Armour et al. (2014), and Burkhauser et al. (2017b) do the same. This assumes that the total value of the insurance dual-eligible individuals receive is not only greater to them than the value for those insured under only one of these programs, but is greater by the average cost of the other program. This may overstate this value to the degree that there is overlap in coverage. But it might understate it to the degree that dual-eligible individuals have higher than average medical expenses relative to those who are only covered by one program. So this value still may be less than the cost dual-eligible individuals incur if they purchase equivalent insurance in

⁷The Medicare and Medicaid risk classes reflect the channel through which benefits were accessed. The Medicare risk class "aged" applies to all persons on Medicare aged 65 or older. The Medicare risk class "disabled" applies to all persons accessing Medicare benefits through the SSDI program. The Medicaid risk class "children" applies to children accessing Medicaid benefits through either traditional Medicaid or a state's Children's Health Insurance Program (CHIP). The Medicaid risk class "adults" applies to all adults under the age of 65 accessing Medicaid benefits. The Medicaid risk class "aged" applies to all persons accessing Medicaid aged 65 or older. Lastly, the Medicaid risk class "disable" applies to all persons accessing Medicaid benefits due to their qualification for SSI benefits. (See Burkhauser, Larrimore and Lyons (2017) for a more complete discussion of this issue.)

the market.⁸

Prior to 1979, the CPS contains no information on the value of health insurance benefits, and no direct information on coverage of health insurance from any source. Thus, to calculate income under this definition we must impute both receipt and market value of insurance from all three sources: Medicaid, Medicare, and employer-provided health insurance. See the Appendix for details on this procedure.

By including health insurance as a source of income we implicitly assume that recipients value these resources at their full market value. This is controversial as health insurance is not a fungible resource and recipients may value health insurance at much less than its market value (Finkelstein et al., 2018). However, excluding the value of health insurance implicitly assumes that these resources do not make recipients better off. Thus, our final two comprehensive measures may be viewed as lower- and upper-bounds on income. Any intermediate valuation of health insurance would lead to an estimate between these two measures.

⁸With the implementation of the Affordable Care Act of 2010, this may no longer be the case, since insurance companies, beginning January 1, 2014, are no longer permitted to adjust their premiums based on pre-existing conditions. However, for the years included in this study insurers could deny insurance to those with pre-existing conditions and/or charge such individuals higher premiums. (See Burkhauser et al. (2017b) for a more complete discussion of this issue.)

3.3 Results

3.3.1 Trends in Median Income

The earliest starting point for CPS-based income measures that include both in-kind transfers and taxes is 1979, as this is the first year that the Census Bureau provides measures of in-kind transfers. As can be seen in Figure 2, using our estimates of in-kind transfers and taxes allows us to extend these income series to 1967 using CPS data, and to 1959 using decennial Census data.

Using 1979 as a base year and adjusting for inflation, Figure 2 first replicates the trends found by Armour et al. (2014) for 1979-2007 for the three most studied measures of income: a Piketty and Saez measure of the market income of tax units (series 1), a measure of household size-adjusted pre-tax post-transfer income of persons found in most early studies of income inequality using CPS data (series 2), and a measure of household size-adjusted post-tax post-transfer income of persons including in-kind transfers (excluding health insurance) found in more recent studies of income inequality (series 3). For the post-tax post-transfer plus in-kind transfers series, we assume that there are no in-kind transfers in 1959.⁹ Our gray bars indicate official NBER recession years.

⁹This is only approximately correct. While the Food Stamp Act of 1964 launched the food stamps program, there was a pilot program from 1961-1964. Housing benefits began with the Housing Act of 1937, but benefits were small prior to the Department of Housing and Urban Development Act of 1965. For instance, total outlays were \$77 million in 1959, rose to \$313 million in 1966, and to over \$1 billion by 1970 in 2012 dollars (See OMB 2016). Likewise, the school lunch program began in 1946 and was expanded and modified several times in the 1960s. The National School Lunch Program (NSLP) is somewhat larger, with expenditures of \$225.8 million in 1960 and \$565.5 million in 1970 (USDA 2013). The relatively small size of the benefits in these programs suggests attempting to estimate their exact value in 1959 would only minimally impact our estimates. For our primary programs of interest, a predecessor program to Medicaid (Kerr-Mills) began in 1961, but there were otherwise no Medicaid or Medicare benefits in 1959. By 1967, the first year for which we have CPS estimates, the programs' combined expenditures were over \$8 billion in 2012 dollars.

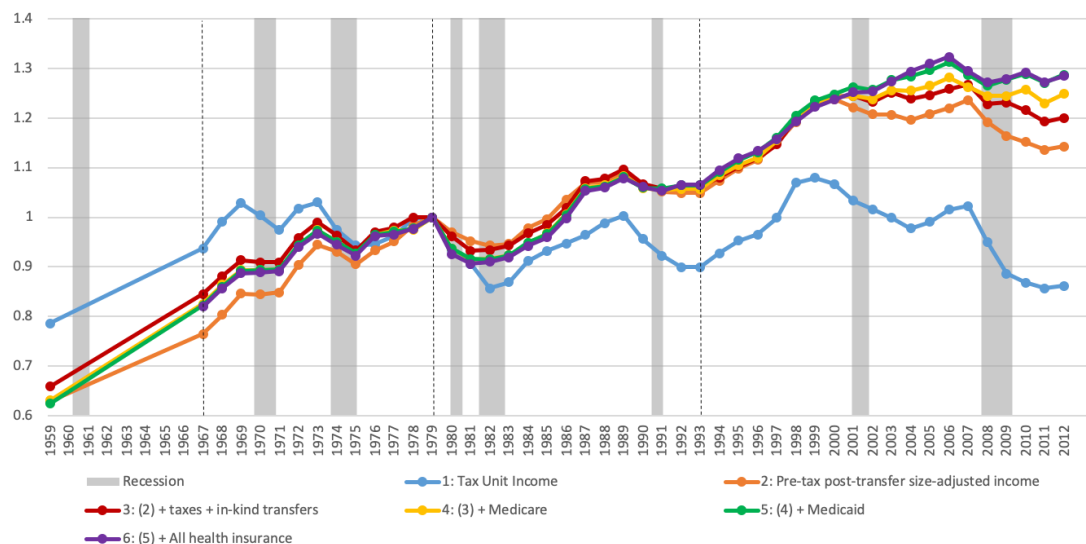


Figure 3.2: Alternative Measures of Median Income Normalized to 1979 Levels (1959-2012). *Sources:* Authors' calculations using the March CPS, NHEA, White House Budget Historical Tables, Statistical Abstracts of the U.S., Census Bureau population estimates and poverty thresholds, USDA Child Nutrition Tables, and 1972-1973 CEX. Taxes calculated using NBER TaxSim. *Notes:* Median income trends normalized to one in 1979 with NBER recession dates in gray. In keeping with previous work (Armour et al., 2014; Burkhauser et al., 2012b; Larrimore et al., 2015), "1: Tax Unit Income" measures the size-unadjusted income of tax units. Series 2-6 are size-adjusted using the square-root of household size. To account for a change in CPS survey methods, it is assumed that all change in income between these years was due to the change in survey methods, and prior years are adjusted accordingly. As we do not have estimates for the value of employer-provided health insurance in 1959, we begin the last measure in 1967. See Appendix Figure 4 for series extended through 2016.

Over the period 1979-2012 that previous survey-based studies have focused on median market income of tax units is quite sensitive to the booms and busts of business cycles. However, with the exception of the 1990s business cycle, when it increased between peak years 1989 and 1999, median market income of tax units trended downward and has fallen precipitously since 2007. In 2012 it was nearly 14 percent below its 1979 peak. See Appendix Figure 4 for series extended through 2016.

In contrast, the median household size-adjusted pre-tax post-transfer income of persons rose over the 1980s and 1990s business cycles and remained at about the same level during the 2000s business cycle, which ended in 2007. While it fell over the Great Recession and its aftermath, it was still more than 14 percent above peak year 1979 in 2012.

When government taxes and in-kind transfers are considered, the downward trend in median market income is further offset. Median income increases in all three business cycles and, while it also fell over the Great Recession and its aftermath, it was still 20 percent above peak year 1979 in 2012.

When we add Census Bureau estimates of Medicare and Medicaid to this measure of income, median income increases even more over the three complete business cycles since 1979, and falls only slightly over the Great Recession and its aftermath. When Medicare is added (series 4) median income is still 25 percent higher in 2012 than in 1979. When Medicaid is added (series 5) median income is 28 percent higher in 2012 than in 1979.

When we add Census Bureau estimates of employer-provided health insurance (series 6) there is little change in median income relative to our previous

measure. Median income increases over all three business cycles and is 28 percent higher in 2012 than in 1979. Note that in our comparison of trends in the income of the median person using these alternative measures of income, that median person will vary.

But how does our understanding of the start of this decline in median market income and its offset via government taxes and transfers change when we use our new estimates of taxes and in-kind transfers to extend our survey-based measures of median income back to 1959?

Between business cycle peak years 1959 and 1969, median market income of tax units rose from 78.6 to 102.9 percent of its 1979 business cycle level. But peak-to-peak business cycle 1959-1969 would prove to be the last with substantial secular growth in median market income. Business cycle peak year 1973 only barely reached its 1969 counterpart (103.0) and median market income would not exceed its 1969 level again until 1998 and has been below its 1969 level since 2002. Hence, the secular decline in median market income of tax units found in studies beginning in 1979, and therefore first observed over the two peak years of the 1980s business cycle, was in fact an extension of a secular decline beginning in 1969.

Like market income, median household size-adjusted pre-tax post-transfer income of persons also grew between business cycle peak years 1959 and 1969—from 62.9 to 84.6 percent of its 1979 business cycle level. But unlike market income of tax units, median household size-adjusted pre-tax post-transfer income of persons continued to grow during the 1970s business cycle, reaching its then all-time high in 1979 (100 percent, as this is our index year).

Our disposable income measures that subtract government tax and add government in-kind transfers by assumption are all at the same level in 1959, since we assume there were no in-kind transfers in 1959 and we only begin to record them in 1967. But they will vary somewhat in Figure 2 since we are normalizing all of their yearly levels to what they were in 1979 and the levels of these three income measures did vary in 1979 but have been normalized to 100.0. Nonetheless, Figure 2 shows that they all grew faster than median market income since they are all at a lower level in 1959 relative to 1979 than is median market income. Like median pre-tax, post-transfer income, 1979 was also an all-time high for each of these disposable income measures. This suggests that while the majority of growth in these measures of income between 1959 and 1969 was driven by the large increase in median market income, the growth in the size of government taxes and transfers for the median American began to offset the secular declines in median market income in the 1970s.¹⁰

Hence, a measure of income that focuses solely on market income of tax units, as a measure of the resources available to the median American from 1959 to 2012, will dramatically understate how these resources have changed over that period. The growth in the redistribution of market income via government tax and transfer policies dating back to the Great Society has not only mitigated the cyclical decline in median market income during recessions but has, more importantly, mitigated the secular stagnation of median market income since 1969.

¹⁰Just as the market value of Medicare and Medicaid should be included in measures of income, so too should the value of employer-provided health insurance. But we do not do so in our analysis of income between 1959 and 1969. Unlike the case for Medicare and Medicaid in which their value was zero in 1959, we suspect that the value of employer-provided health insurance was non-trivial. But we have not been able to find a plausible way to capture that value in the aggregate and assign it to our 1959 population in the decennial Census. We have however done so beginning in 1967 for our CPS population and will discuss those results in more detail in the Appendix.

3.3.2 Trends in Income Across the Distribution

The importance of taxes and transfers over this entire period can be seen in more detail in Table 1. Row 1 (Panel A) reports cumulative median income growth, controlling for inflation, for the entire period of our data from 1959 through 2012 for each of our income definitions based on values underlying Figure 2.

Table 3.1: Income Growth using Alternative Measures of Income by Quintiles for Alternative Time Periods

			Household Size-Adjusted Post-tax Post-transfer		
	Market Income of Tax Unit	Household Size-Adjusted Pre-tax Post-transfer income	Income plus in-kind income	Income plus in-kind income and Medicare	Income plus in-kind income and Medicare/ Medicaid
Panel A: 1959-2012					
Median	10.5%	81.8%	82.0%	97.9%	106.0%
Q1	-45.7%	75.2%	116.2%	148.0%	195.4%
Q2	3.4%	65.7%	75.1%	96.2%	112.5%
Q3	12.4%	82.1%	82.5%	97.7%	105.6%
Q4	40.5%	98.9%	90.8%	100.7%	104.3%
Q5	93.8%	128.3%	105.4%	110.8%	111.9%
Top 5%	116.0%	151.2%	123.6%	127.1%	127.6%
Panel B: 1959-2007					
Median	30.1%	96.7%	92.2%	100.1%	106.0%
Q1	15.3%	100.3%	128.9%	156.0%	196.4%
Q2	41.5%	83.3%	85.9%	100.0%	112.5%
Q3	31.0%	96.3%	91.9%	100.2%	106.2%
Q4	54.5%	109.3%	97.1%	101.9%	104.6%
Q5	105.6%	133.1%	107.6%	110.4%	111.3%
Top 5%	127.7%	150.5%	121.6%	123.5%	124.0%

Sources: Authors' calculations using the March CPS, NHEA, White House Budget Historical Tables, Statistical Abstracts of the United States, Census Bureau population estimates and poverty thresholds, USDA Child Nutrition Tables, and 1972-1973 CEX. Taxes calculated using NBER TaxSim.

Notes: Series covers 1959-2012. 1959 and 2007 are earliest and latest business-cycle peak years, respectively.

In the rest of the rows, it shows how cumulative income growth has varied

over the entire income distribution. It does so by estimating cumulative mean income growth for each quintile and the top 5%, for each of our income definitions. However, since 2012 is not a peak year in a business cycle, and thus the interpretation for income growth ending in that non-peak year also contains cyclical effects (the cyclical effects of the Great Recession and its aftermath between 2007 and 2012), we will primarily focus on trends in income growth between business cycle peak years 1959 through 2007. Those values are reported in Table 1 (Panel B). Note that the quintile composition is not constant across measures—that is, persons may switch quintiles for different measures of income.

As was the case in Figure 2, the first column in Table 1 presents the growth in cash-market income of tax units and is not size-adjusted. The remaining four columns use the household as the sharing unit and the person as the unit of analysis. First-column results in Table 1 (Panel A) are consistent with those of Piketty and Saez (2003) and Atkinson et al. (2011). When focusing solely on market income in Panel A, the rich get richer, the poor get poorer, and median income has been stagnant since 1959. Mean income increased among the top 5% of tax units by 116 percent between 1959 and 2012 while declining by 45.7 percent for those in the bottom quintile and increasing by only 12.4 percent for those in the middle quintile (this value is close to the 10.5 percent growth found in the first-column of Panel A that reports the median value from the entire distribution rather than mean growth of the middle quintile based on values reported in Figure 2).

Part of the slow growth captured between these years is the result of comparing a peak year, 1959, with 2012—a year median income had only begun

to recover from the Great Recession and its aftermath, regardless of how it is measured. Growth rates for all quintiles are higher when peak year 1959 is compared with peak year 2007, as can be seen in Panel B. But the discrepancy in growth is still dramatic across the distribution. While now even the lowest quintile has positive growth in market income, it is quite small over this 48-year period, as is the growth in the middle quintile.

But as we change our measure of income across the remaining columns in Panels A and B to those used in the standard survey-based income and income inequality literatures, the growth in median income and in the mean value of all quintiles increases. Median value increases from 30.1 percent in Panel B to 96.7 percent (the next column) when other cash income including government transfers are added to market income and the sharing unit is expanded to the household from the tax unit, and the unit of analysis is the person rather than the tax unit, and income is adjusted to account for the number of people in the household. When taxes are subtracted from income and in-kind transfers (but not the value of Medicare and Medicaid) are included, the growth in median income is approximately the same (92.2 percent). But when the value of Medicare and then both Medicare and Medicaid are considered (the next two columns), median income increases to 100.1 and 106.0 percent.

The pattern of increased growth as government taxes and transfers are added to market income is the same for all quintiles but to different degrees. For the bottom quintile income growth increases from 15.3 percent to 100.3 percent when looking at pre-tax post-transfer income—an increase greater than that found in the next two higher quintiles. When taxes and in-kind transfers (but not Medicare or Medicaid) are included, income growth in the bottom quin-

tile increases to 128.9 percent—an increase greater than that found in all other quintiles and the top 5%. The growth rate rises to 156.0 percent when the market value of Medicare (which was zero in 1959) is included and 196.4 percent when the market value of Medicaid is included. Hence, standard measures of disposable income that exclude the market value of Medicare and Medicaid, as reported in column 3, substantially understate the growth in real income across the entire distribution but especially in the bottom quintile. While public insurance had little effect on median income as shown in Figure 2, it is much more important for the bottom of the income distribution as this is where most recipients are concentrated, particularly for Medicaid.

3.3.3 A Closer Look at Income Growth from 1959 to 1979

In Table 2, rather than focusing on 1959 through 2007, we now, for the first time, use the methods developed for Table 1 to focus on how alternative measure of income affect measured growth from 1959 to 1969 and from 1959 to 1979.

As can be seen in Panel A, cumulative secular market income growth between business cycle peak years 1959 and 1969 is dramatically different from all subsequent business cycles. Not only does cumulative median market income of tax units increase by 30.9 percent but mean growth in the bottom quintile is higher than in all other quintiles as well as in the top 5%.¹¹ Although growth in median income increases from 30.9 to 42.8 percent as we add government transfers (including Medicare and Medicaid) and subtract taxes, growth in the bottom quintile rises even more, from 47.3 percent to 113.7 percent. And while

¹¹Median market income fell or was stagnant over all subsequent business cycles with the exception of 1989-1999. However cumulative growth over that 10-year period was only 7.9 percent—less than 1/3 of cumulative growth between 1959-1969.

Table 3.2: Income Growth using Alternative Measures of Income by Quintiles for Alternative Time Periods

			Household Size-Adjusted Post-tax Post-transfer		
	Market Income of Tax Unit	Household Size-Adjusted Pre-tax post-transfer income	Income plus in-kind income	Income plus in-kind income and Medicare	Income plus in-kind income and Medicare/ Medicaid
Panel A: 1959-1969					
Median	30.9%	34.6%	38.6%	41.6%	42.8%
Q1	47.3%	66.9%	80.8%	98.4%	113.7%
Q2	38.7%	38.6%	45.2%	50.5%	54.3%
Q3	31.8%	34.8%	38.2%	41.3%	42.7%
Q4	34.5%	34.0%	34.1%	36.6%	37.3%
Q5	39.2%	35.3%	29.7%	31.3%	31.6%
Top 5%	44.3%	39.5%	30.1%	31.2%	31.3%
Panel B: 1959-1979					
Median	27.2%	59.1%	51.7%	54.5%	55.4%
Q1	50.2%	82.2%	98.8%	109.8%	126.1%
Q2	39.3%	58.5%	54.9%	59.6%	63.3%
Q3	28.0%	59.8%	51.1%	53.6%	54.9%
Q4	41.0%	62.0%	47.5%	49.0%	49.7%
Q5	57.3%	64.1%	37.9%	38.7%	38.9%
Top 5%	66.6%	68.4%	33.7%	34.1%	34.2%
Panel C: GINI					
1959	0.516	0.387	0.360	0.360	0.360
1969	0.498	0.370	0.321	0.312	0.304
1973	0.511	0.370	0.317	0.308	0.298
1979	0.530	0.384	0.316	0.310	0.302
2007	0.557	0.430	0.370	0.356	0.338
2012	0.580	0.439	0.371	0.354	0.334

Sources: Authors' calculations using the March CPS, NHEA, White House Budget Historical Tables, Statistical Abstracts of the United States, Census Bureau population estimates and poverty thresholds, USDA Child Nutrition Tables, and 1972-1973 CEX. Taxes calculated using NBER TaxSim.

Notes: We break up growth across periods by intervals for business cycle peaks in 1959-1969, as well as growth over the entire early period 1959-1979. Gini values are for all business cycle peaks.

market income growth among the top 5% is next highest when only the market income of tax units is considered, once taxes and transfers are considered top 5% growth is the lowest of all groups.

Subsequent growth over the two business cycles of the 1970s was much lower across all measures of income and across the entire income distribution. Table 2 Panel B reports cumulative growth from business cycle year peak 1959 to business cycle peak year 1979. Except for median market income (and mean market income growth in the middle quintile) cumulative growth between 1959 and 1979 is greater than between 1959 and 1969 across all five of our income measures.

But the great majority of that growth occurred between 1959 and 1969. The dramatic 47.3 percent increase in the market income of the bottom quintile between 1959 and 1969 only increases to 50.2 percent over the total period 1959 to 1979. Market income growth is similarly anemic in the second quintile and is lower in 1979 than in 1969 in the middle quintile. In contrast, the top 5% now registers the highest growth in market income between 1959 and 1979, and the top quintile's market income growth rates is also now greater than the bottom quintile's growth rates over that period. Because the importance of government taxes and transfers continues to increase in the 1970s, overall cumulative growth rates in the other income categories are uniformly higher—but again, most of that growth occurs in the 1960s rather than the 1970s.¹²

¹²As discussed in the Appendix, between 1959 and 1979 our most precisely measured concept of income is post-tax post-transfer income since the CPS and the decennial Census were designed to capture this measure of income. All our other income measures require some estimation on our part. Hence we were faced with a trade-off between more precisely measuring a poorer concept of income or accepting a more imprecise measure of a better concept of income. But this trade-off of greater imprecision for a better conceptual measure of income especially shows up in our measure of the income of the bottom quintile. This is the case because in 1959 household size-adjusted post-tax post-transfer income for persons in the bottom quintile was only \$4,642 (in 2012 dollars). Such a low base means moderate changes can lead to substan-

This pattern of differential income growth across the distribution is captured in Table 2 (Panel C), which shows how Gini values change over each of the four business cycle peak years from 1959 to 1979 for each of our income measures, and then for business cycle peak year 2007 and for 2012. For all five business cycle peak years and for 2012, Gini values are highest (most unequal) for market income of tax units and fall as we increasingly take into account government taxes and transfers. This is reassuring since one of the goals of government tax and transfer policy is to transfer market income from higher income household to lower income households, and this occurred in all years. But what our new data now show is how Gini value trends have changed across each of these income measures since 1959. The Gini value for the market income of tax units was 0.516 in 1959 and fell in 1969 to 0.498. But the decline in median market income between 1969 and 1979 (seen in Figure 2) together with the growth in market income at the top of the distribution resulted in a substantial increase in the Gini value in 1979 to 0.530—a value in excess of its value in 1959. Gini values subsequently increased to 0.557 in 2007 and to 0.580 in 2012.

The Gini pattern for pre-tax post transfer income is similar but the growth in government in-cash programs during the 1970s offset to some degree the rise in market income inequality observed for tax units. Gini values decline between 1959 and 1969 and rise over the 1970s. But the Gini value in 1979 is still below its 1959 value—0.384 vs. 0.387. However, by 2007 it had risen to 0.430 and by 2012 to 0.439.

But this comparison of how government in-cash programs offset rising inequality in market income misses the importance of tax policy (disposable in-

tial growth. Furthermore, in measuring the cumulative effect of all the Great Society programs and in-kind transfers we assume that these program values were zero in 1959 (see footnote 10 above).

come) and the increasing importance of in-kind transfers in further reducing income inequality. When we include these two sources of income in column 3, the rise in market income inequality over the 1970s is now shown to be completely offset by tax and transfer policies. In 1959 the Gini value of this measure of income was 0.360—a value not much different from the 0.387 for pre-tax post-transfer income. But unlike our pre-tax post-transfer measure, the Gini value for our disposable income plus in-kind transfer measure of income not only falls to 0.321 in 1969 but continues to fall to 0.316 in 1979, despite the rise in market income inequality. Nonetheless, the Gini value of this measure of income has subsequently increased to 0.370 in 2007 and to 0.371 in 2012. Hence while this more appropriate measure of the after-tax resources available to Americans is substantially more equal than is a Gini value based on post-tax post-transfer income and that difference has grown dramatically since 1959, it still suggests that government tax and transfer policies have not been able to offset the substantial increase in market income inequality since 1959.

We now turn to the importance of including the market value of Medicare and Medicaid in measures of median income and income inequality, because, as illustrated in Figure 1, expenditures on these two programs have been growing over time and now exceed OASDI and SSI expenditures. In 1959 neither Medicare nor Medicaid existed so income inequality including their market income value was the same as in our previous disposable income measure—a Gini value of 0.360. Adding the market value of Medicare (column 4) and then Medicaid (column 5) further offsets the rise in market income inequality in the 1970s. Despite the increase in market income inequality between 1969 and 1979, the income inequality of Americans not only fell between 1959 and 1969 (from 0.360 to 0.304) but also continued to fall between 1969 and 1979—from 0.304

to 0.302—once Medicare and Medicaid are included. More importantly, while inequality using this measure of income has also risen as market income inequality has risen, its Gini value was 0.338 in 2007 and 0.334 in 2012, still well below 1959 levels.¹³

Our combined results are largely consistent with the findings of Moffitt (2015) who focuses on government spending on welfare programs. He found significant growth in the early 1970s, slow growth in the late 1970s to the mid-1980s, and higher growth from the late 1980s onward. Our findings show that these are roughly the periods during which in-kind transfers including Medicare and Medicaid (which he does include in his analysis), largely mitigated income inequality in market income.

3.3.4 The Relationship between Mean and Median Income since 1959

A new literature has developed that has attempted to capture the long term relationship between aggregate measures of growth using National Accounts data—e.g., per capita Gross Domestic Product—and the real income of the median person (median real GDP) since it is argued to be a more appropriate measure of the resources available to the average person than is a measure of mean income like per capita GDP that can rise even when most of the income growth

¹³We are first able to estimate the market value of employer-provided health insurance in 1969. When we add this source of income to column 5 the Gini value is 0.290 in 1969 and then falls to 0.285 in 1973 before rising to 0.301 in 1979. They then rise to 0.338 in 2007 and to 0.334 in 2012. Because we are unable to measure the value of employer provided health insurance in 1959 we cannot make comparison of from 1959 to subsequent years as we do for our other five measures of income. But it is unlikely that its presence would offset the patterns we show in column 5. See the Appendix for a discussion.

is at the top end of the distribution. While this may be conceptually appropriate, operationally it is not possible to directly capture median real GDP using National Accounts data alone. To solve this problem, researchers have turned to either survey or administrative tax record data or some combination of the two to capture trends in median income. But it is critical that the sources of income used in the National Accounts match those used in the survey or administrative tax record data or “like is not compared to like”.¹⁴ (See Nolan, Roser and Thewissen, 2016 for a review of this literature)

Gordon (2016 Table 18.4) uses such a measure of median income derived from survey data in his estimates of median real GDP from 1975 to 2012. It is based on CBO estimates using CPS data statistically matched to income tax record data. But while the CBO has been including the market value of Medicare and Medicaid in its measures of income since 2012, these values are not included in previous years. More problematic, for earlier years Gordon estimates median real GDP using top income data from the World Top Income Database (Alvaredo et al., n.d.). However, these data contain information on the taxable market income of tax units as it comes from the series developed by Piketty and Saez (2003).

In Figures 3 and 4 we use our new data set to show the problem of comparing levels and trends in real GDP per capita to levels and trends in real market income of the median tax unit. In Figure 3 we compare GDP per capita income from 1959 to 2012 taken from the Bureau of Economic Analysis to the real market income of the median tax unit based on the same CPS/decennial Cen-

¹⁴ Atkinson et al. (2015) use data from EU-SILC country surveys from 2004 to 2011 to demonstrate the problems of replicating National Accounts measures of mean income with survey data. To the degree that the survey data captures mean income based on National Accounts concepts of income, it allows researchers to compare such a measure with a median income measure, which can be captured in survey data.

sus data that underlies the values reported for this income measure in Figure 2. Using such a measure shows that between 1959 and 1969 real GDP increased from \$18,312 to \$24,874 (35.8 percent) while the real median market income of tax units rose from \$14,977 to \$20,044 (33.8 percent). But since then the increase in real mean GDP has substantially outpaced the growth in median market income.

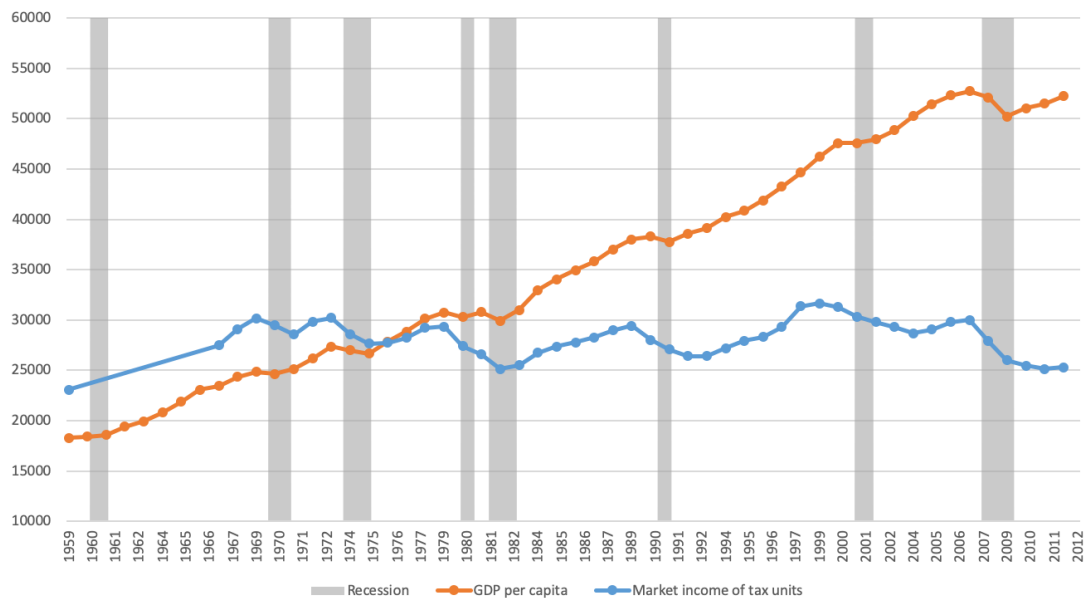


Figure 3.3: Comparison of Real GDP per Capita and Real Median Taxable Market Income of Tax Units (in 2012 dollars). *Sources:* Median market tax unit income from authors' calculations using March CPS and the 1960 decennial Census. Income per-capita from the Bureau of Economic Analysis NIPA Table 7.1. *Notes:* The series showing the share of income going to the top 1% of income earners is adjusted for the years 1986-1988 to account for large tax changes, in a similar fashion as our CPS series is for the 1992-1993 survey redesign. The series for median tax unit income includes CPS data for 1967-2012 and Census data for 1959. Series are converted to 2012 dollars. NBER recessions are shown by gray bars.

Their relative levels of growth can best be seen in Figure 4 where we use the same data but normalized to 1.00 in 1979 to show differences in growth.

Between 1959 and 1969 median market income increased at approximately the same rate as real GDP. But since then GDP per capita has increased substantially while median market income has trended downward.

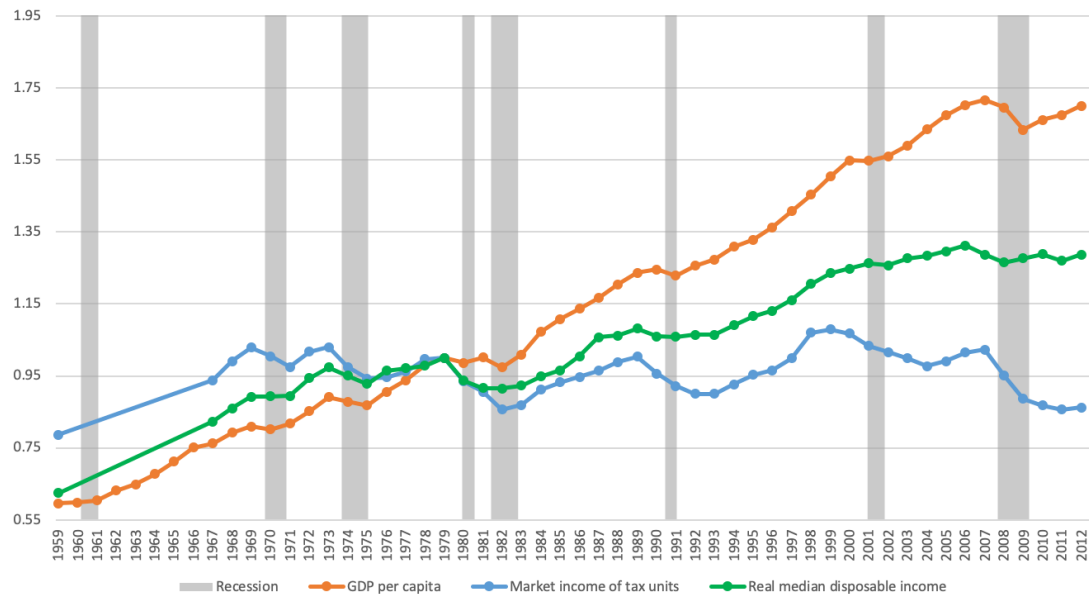


Figure 3.4: Real GDP per Capita, Real Median Taxable Market Income of Tax Units and Real Median Disposable Income including Medicare and Medicaid of Household Size-Adjusted Income of Persons Normalized to 1979. *Sources:* Median market tax unit income from authors' calculations using March CPS and the 1960 decennial Census. Income per-capita from the Bureau of Economic Analysis NIPA Table 7.1. *Notes:* Same series as in Figure 2 with values normalized to income year 1979.

But this picture changes when we compare it to our fullest measure of income in Figure 2 — household size-adjusted disposable income including the market value of Medicare and Medicaid of persons, a measure more in line with GDP than market income alone. While growth in real GDP especially since 1973 has outpaced even this fuller measure of household size adjusted disposable income, the difference is much less than compared to median market income of

tax units especially since 1973.

3.4 Summery and Conclusion

Although still controversial, a growing CPS-based literature beginning in 1979 demonstrates that excluding the market value of in-kind transfers including Medicare and Medicaid from measures of income substantially understates the importance that government tax and transfer policies have increasingly played in offsetting market income inequality and in disproportionately providing resources to those in the bottom half of the income distribution. But data limitations have prevented such analyses for earlier years. To overcome these data limitations, we use March CPS data (income years 1967-2012) and decennial Census data (decennial income years 1959-1989) to create common yearly source of income categories, including estimates of the market value of in-kind transfers, back to 1959.

Using these data we provide a fresh look at the twenty-year period 1959 to 1979. We find that over the business cycle of 1959-1969 cumulative real median market income rose substantially (30.9 percent) and the market income of the bottom quintile rose faster (47.3 percent) than that of the top 5% (44.3 percent) resulting in a decline in market income inequality as a growing economy “lifted all boats.”

Furthermore, the launch of New Frontier and Great Society programs, which were heavily tilted toward the bottom part of the income distribution over this decade, as well as the maturing of OASDI led to cumulative increase in mean growth for the bottom quintile of 66.9 percent using standard pre-tax

post-transfer measures of income, 80.8 percent when taxes and some in-kind transfers are considered and 113.7 percent when Medicare and Medicaid are included.

But we then find that median market income fell between 1969-1979. Thus we show that the stagnation in median market income captured in the survey-based literature since 1979 effectively began after peak business cycle year 1969. However, this drag on resources was offset by government tax and transfer policies which resulted in a cumulative increase in median income between 1969 and 1979 regardless of how it was measured.

Using 1959 as the starting point for studies of median income and income inequality reinforces the importance of distinguishing questions about how market income is distributed across tax units (the measure of income in the tax record-based top income literature) and how resources (market income plus the net of government taxes and transfers) are distributed across people living in households. While we demonstrate that the stagnation of median market income effectively began after 1969, we also show that government tax and transfer policies have transformed a 10.5 percent cumulative increase in market income between 1959 and 2012 into a 106.0 percent increase when taxes and transfers are more fully accounted for.

Conventional measures of median income and income inequality that exclude the market value of in-kind transfers including Medicare and Medicaid will substantially understate the success of government policies in offsetting the stagnation in median market income and the rise in market income inequality since 1969.

APPENDIX A
CHAPTER 1 APPENDIX

A.1 Medicaid and CHIP Expansions

The Omnibus Budget Reconciliation Acts (OBRA) of 1986 and 1987 were both state optional. OBRA 1986 permitted states to grant eligibility to children under age two in families with incomes below 100 percent of the FPL. OBRA 1987 allowed states to immediately cover children under age 5 (born after September 1983) and extend eligibility for infants up to 185 percent of the FPL. However, OBRA 1987 required children born after September 1983 be eligible regardless of family structure if their family met AFDC income standards. The Medicare Catastrophic Coverage Act (MCCA) of 1988 required expanded coverage for infants to be phased in by July 1990. OBRA 1989 required extension of coverage of children under age six to 133 percent of the FPL, and OBRA 1990 extended coverage to children under 19 born after September 1983 to 100 percent of the FPL. These expansions create sharp increases in eligibility within age groups as children born after the September 1983 cutoff age, resulting in a gradual increase in the fraction of children covered by federal expansions throughout the 1990s. In addition to the Federally mandated expansions, my eligibility measures also include variation due to state-level expansions beyond the federally required minimum coverage thresholds.

CHIP was created in 1997 to further expand health insurance coverage for children, and by July 2000 all 50 states and Washington D.C. had implemented CHIP programs. States had the option to create a CHIP program separate from Medicaid, combine the two programs, or expand their existing Medicaid pro-

gram. CHIP differs from Medicaid to some degree for several reasons. Funding for CHIP was through block grants, thus capping the funding available. However, Federal match rates were higher than under Medicaid. States also had more flexibility in the design of CHIP programs from federal guidelines.¹ The programs are otherwise similar and in my analysis I treat CHIP expansions as Medicaid, as has other recent work using both programs (Gross and Nottowidigdo, 2011; Cohodes et al., 2016). However, I treat CHIP as state-optional Medicaid expansions. While all states created CHIP programs, the timing and eligibility thresholds were largely left to the discretion of the states. CHIP programs expanded eligibility further up the income distribution to families with incomes up to of 200 or 300 percent of the FPL, and in many states expanded coverage levels for older children. For more details on these expansions, see Shore-Sheppard (2003) and Buchmueller et al. (2016).

A.2 Income and Medicaid Eligibility

As shown in Figure 1.4, beginning around 1996 measured actual and simulated Medicaid eligibility diverge. Their contemporaneous passage makes PROWRA and SCHIP prime suspects for driving the divergence, but in fact rapid income growth in the late 1990s drives this divergence. Figure A.1 shows trends in average nominal income in the samples used to calculate actual and simulated eligibility. Similar to the series in Figure 1.4, the average incomes in the actual and simulated series are similar in levels and trends from 1980 until 1996, diverge between 1996-2000, and maintain different levels but similar trends thereafter. For

¹There are other differences between Medicaid and CHIP. For instance, CHIP contained anti-crowd-out measures such as denying eligibility to children with another source of insurance while Medicaid does not. For more details on Medicaid expansions, see Shore-Sheppard (2003).

instance, the average difference between simulated and actual income is 2.8% prior to 2000, but is 13.2% from 2000 on. The large increases in actual income observed in the late 1990s lead to lower eligibility levels, as families have higher incomes and thus a smaller fraction fall below the income eligibility thresholds. Moreover, the explanation of income growth driving the divergence in eligibility measures is appealing because the direction of the differences in income map predictably onto the direction of the difference in eligibility. Between 1980 and 1996, income is slightly higher in the simulated sample than the actual sample (with the exception of 1990 when they are identical), and in these years actual eligibility is slightly higher than simulated eligibility, which is expected if the actual sample has lower income. The direction reverses after 1996 when actual income surpasses income of the simulated sample, and eligibility of the simulated sample rises above eligibility in the actual sample.

The degree of similarity between the actual and simulated income series between 1979 and 1996 may be surprising, given that real GDP growth was relatively strong over this period. For instance, between 1979 and 1990 (both business cycle peak years), average annual GDP growth was 3%, and between 1980-1996 average annual GDP growth was 2.9% (U.S. Bureau of Economic Analysis, 2018).² If this income growth were spread evenly across the income distribution, then income in the actual and simulated samples would diverge, as by construction real income in the simulated sample is constant.

In reality, growth over the 1979-1989 business cycle was highly unequal. Burkhauser et al. (2012b) show that average pre-tax pre-transfer income of the

²Technically 1981 is also a peak year because there were NBER defined recessions from January 1980-July 1980 and July 1981-November 1982. However, because this expansion period was brief and the March CPS is conducted only once annually, prior research using the CPS to analyze business cycles has combined these two cycles into a single cycle from 1979-1990. For example, see (Burkhauser et al., 2012b).

bottom three quintiles (where eligibility for Medicaid is concentrated) of the distribution grew -0.2% , -5.0% , and 0.0% respectively over this period for tax units, and post-tax post-transfer income grew 5.0% , 0.2% , and 6.3% for households (they do not consider families, which would be an intermediate unit of analysis between tax units and households).³ In contrast, from 1989-2000 the same three quintiles had growth rates of 17.8% , 10.8% , and 7.5% respectively for tax unit income and 10.6% , 8.3% , and 10.7% respectively for household income. Considering income growth of the top quintile (where few families are eligible for Medicaid), tax unit incomes grew 17.6% and 14.7% over the two business cycles. Household incomes of the top quintile grew 19.7% and 14.0% over the two cycles respectively. The benefits of economic growth were more evenly distributed during the 1989-2000 business cycle, particularly over the final years of the cycle. While there was substantial economic growth over the 1979-1989 business cycle, these gains were concentrated at the top of the income distribution, and as a result average incomes of the actual and simulated series are similar during this period.

To further test the degree to which income drives the divergence of the two Medicaid eligibility measures, I apply an adjustment to the incomes in the sample measuring actual Medicaid eligibility. Specifically, for each year I calculate the ratio of the average income in the simulated and actual samples.⁴ I then adjust the incomes of every family in every year in the sample used to calculate actual eligibility down by this ratio. In doing so, the income distributions of

³Pre-tax pre-transfer income includes labor earnings and non-labor market income such as interest, dividend, or rents. Post-tax post-transfer income includes the same sources as well as cash transfers from both government and non-government sources. Post-tax post-transfer income is similar to the income measure that would be used to calculate a family's Medicaid eligibility, although the included sources of income do not match exactly. Burkhauser et al. (2012b) do not report other intermediate income definitions.

⁴I use the Unicon variable "faminc", which includes all family income from both earned and unearned sources. Using earned income, Unicon variable "incwag", yields similar results.

the two samples are made roughly equivalent, as this “removes” real income growth from the sample used to calculate actual eligibility. By construction, there is no adjustment for 1990 which is the same in both samples. I then graph this adjusted actual Medicaid eligibility series with my original simulated eligibility series in Appendix Figure A.2 for overall Medicaid eligibility and Appendix Figure A.3 for federal eligibility. Even with this relatively rudimentary adjustment, the two series are even more similar, and the gap in eligibility that occurred in the late 1990s essentially disappears.⁵

A.3 Other Safety Net Programs

I consider four additional safety net programs that are included in the CPS. The National School Lunch Program (NSLP) provides free and reduced price school lunches to children from low-income families. In 2017 the NSLP had total expenditures of \$18 billion. The lesser known Low-Income Home Energy Assistance Program provides in-kind benefits to purchase heating, cooling, or weatherization of homes, and is a smaller program with total funding of only \$3 billion in 2017. It is the only program I consider that does not exist in the CPS for all years of my analysis. It was created in 1980, but was not added to the CPS questionnaire until 1982.⁶

I examine the Supplemental Security Income (SSI) and Social Security Disability Income (SSDI) programs as well, although these programs are qualita-

⁵I obtain similar results when using median rather than mean income. A more complex adjustment could take into account factors such as race or position in the overall income distribution, but is unlikely to substantially improve over this simple adjustment in terms of explaining the gap between actual and simulated eligibility.

⁶The CPS also asks about Women, Infants, and Children (WIC) reciprocity. However, this question is not included in the CPS until 2000, and so I exclude it from my analysis.

tively different from the other six. Both SSI and SSDI have complex additional eligibility criterion because they target primarily individuals with disabilities, and neither target children in low-income families specifically. Although SSI does cover children in some cases, SSDI requires a work history and could only be received by an older adult within a family, and never directly by children. These two programs serve different intended purposes than the other programs I consider, but also provide a substantial source of income to many low-income families, and particularly due to their disability criterion they may be expected to interact with Medicaid eligibility. SSI and SSDI had total expenditures of \$59 billion and \$149 billion in 2017, respectively.

A.4 Simulations of Other Safety Net Programs

One threat to identification would be the rules for other transfer programs being changed concurrently with expansions to Medicaid, or alternatively if there were underlying secular trends in characteristics related to eligibility, correlated with Medicaid, that affect program participation, and are omitted from my empirical specifications. Because of the potential underlying secular trends, simply documenting changes to policies regarding other programs is insufficient, even if those changes are not contemporaneous with Medicaid expansions.

To address this issue, I simulate program eligibility for my outcome programs in addition to simulated Medicaid eligibility. That is, I use the same fixed national sample (the national 1990 CPS sample), and apply eligibility criteria for each state and year to that sample for each transfer program. Just as with Medicaid eligibility, the variation in this measure is due only to statutory changes in

program rules, and not to family level characteristics or secular trends in demographics or economic conditions. Then, regressing these simulated eligibility measures on Medicaid eligibility provides a test as to whether Medicaid expansions are exogenous with respect to policy changes in other transfer programs, and should the results be significant allow me to control for eligibility in these programs by including their simulated eligibility in my controls as a robustness check.

As with my simulations of Medicaid eligibility, I am constrained to simulate eligibility based only on the characteristics observable in the CPS, and as a result must ignore some eligibility criteria such as programs requiring asset tests.⁷ I only simulate eligibility for five of the transfer programs: AFDC/TANF, EITC, SNAP, NSLP, and LIHEAP.⁸ Estimating eligibility for housing subsidies is problematic with the CPS because, among other criteria, eligibility depends on a percentage of median income which is determined by the local public housing agency (PHA). These cover geographic areas that are smaller than a state, and the CPS lacks sufficient sample size and geographic information to calculate incomes within each PHA.⁹ Even with external information on the median income of each PHA, simulated eligibility could be calculated by using the national sample, but no meaningful information on actual outcomes at this geographic level could be measured using the CPS.

I do not simulate SSI and SSDI because both use disability status as a pri-

⁷There are a variety of other criteria that cannot be accounted for that apply to some or all programs, for instance disability status, criminal history, housing history, cumulative amount of time receiving benefits, or income not covered by the CPS, for instance capital gains. Of course previous literature using simulated eligibility has faced the same constraints.

⁸I am thankful to Michiel Paris, as my SNAP eligibility simulations are based on code from Paris (2018). I would additionally thank Jason Cook, who provided me with code that assisted with my AFDC/TANF simulations.

⁹For instance, the state of New York has over 150 separate PHAs.

mary determinant of eligibility, for which there is limited information in the CPS. SSDI also depends on earnings history, which is also not measured in the CPS. However, for both programs the recipients are likely other family members, not children themselves, and they are relatively uncommon in my sample. Thus, simulating them is less important than other programs for which receipt is concentrated in families with children. Both would be interesting avenues of future work.

A.5 Program Participation Results for Other Safety Net Programs

In Appendix Tables A.2-A.4, Columns 8-11 present the effects of Medicaid expansions on participation in the NSLP, LIHEAP, SSI, and SSDI. I consider the NSLP and LIHEAP separate from my main results because these programs are smaller, particularly for LIHEAP. In addition, LIHEAP is not an entitlement program and has relatively limited funding. SSI and SSDI are both large programs, but differ from the primary safety net programs I consider because they do not specifically target low-income families with children, and have substantively different eligibility criteria because both programs depend on some disability criteria, and as a result may be expected to be less sensitive to Medicaid expansions.

Of the four programs, the only program affected by Medicaid expansions is the NSLP. A 10 percentage point expansion in children's overall Medicaid eligibility reduces participation in the NSLP by 0.44 percentage points, or around one percent. The effect of a similar expansion in federal Medicaid eligibility

is only marginally significant, but indicates a 10 percentage point expansion in federal eligibility reduced participation in the NSLP by 0.2 percentage points, or 0.4 percent. This reduction in NSLP participation is consistent given the pattern of results for other programs and labor supply. Relative to other programs eligibility for NSLP is quite generous. Children receive reduced-price school lunches in families with incomes up to 185 percent of the FPL, almost identical to the EITC program which phases out at 182 percent of the FPL as discussed in Section 1.6.1. Like the EITC, the NSLP program has relatively high take-up rates as shown in Appendix Table A.1. Thus, there is less scope for Medicaid expansions to increase take-up of the NSLP, but expansions may still reduce NSLP participation by reducing eligibility.

I find no effect of Medicaid expansions on LIHEAP, SSI SSDI, participation. The results for SSI participation are consistent with the findings of Levere et al. (2018) and Shore-Sheppard (2008) who found similar, tightly estimated null effects on SSI. The null results for SSDI in particular are reassuring, as few families with children receive benefits from this program and the beneficiaries could never be children themselves, as individuals must have a substantial work history to be eligible for SSDI benefits in addition to having a disability.

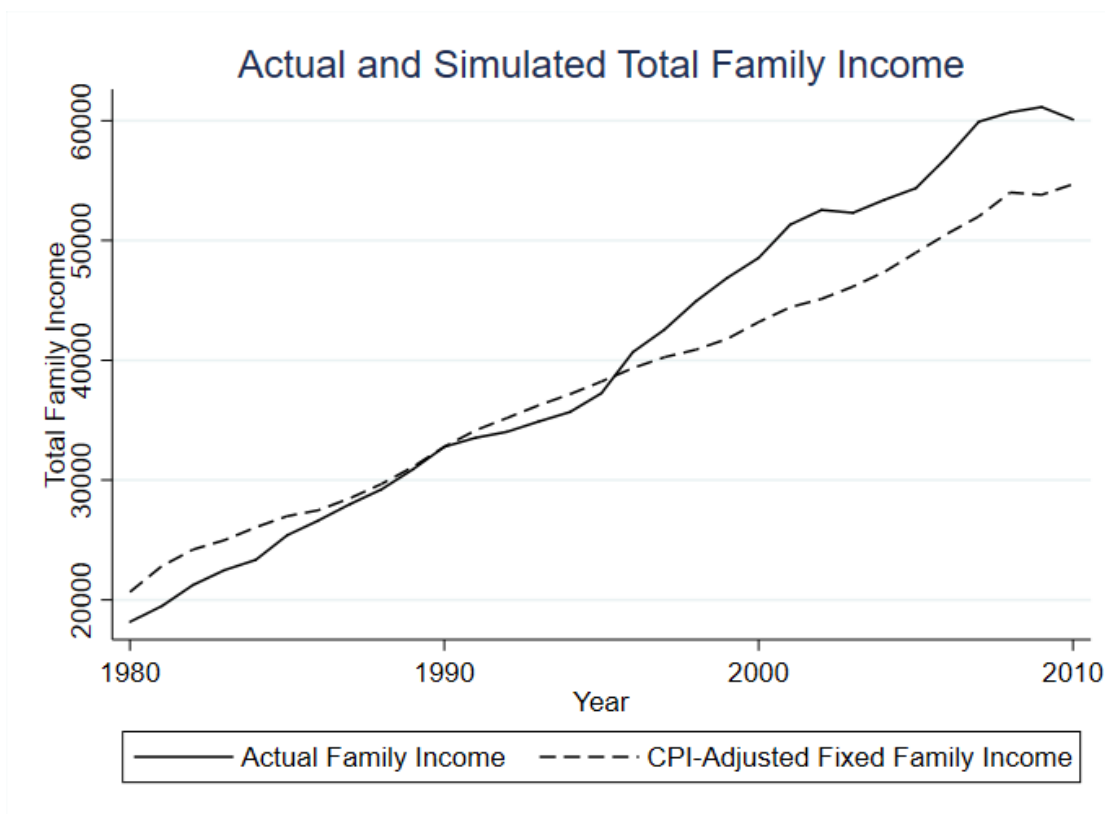


Figure A.1: Author's calculations using the 1979-2011 March CPS and Medicaid eligibility rules for each state and year. Each line shows average eligibility for children ages 0-17, by race. Simulated eligibility uses a fixed sample of the 1990 CPS. Incomes for the fixed sample are adjusted to each year using the CPI-U.

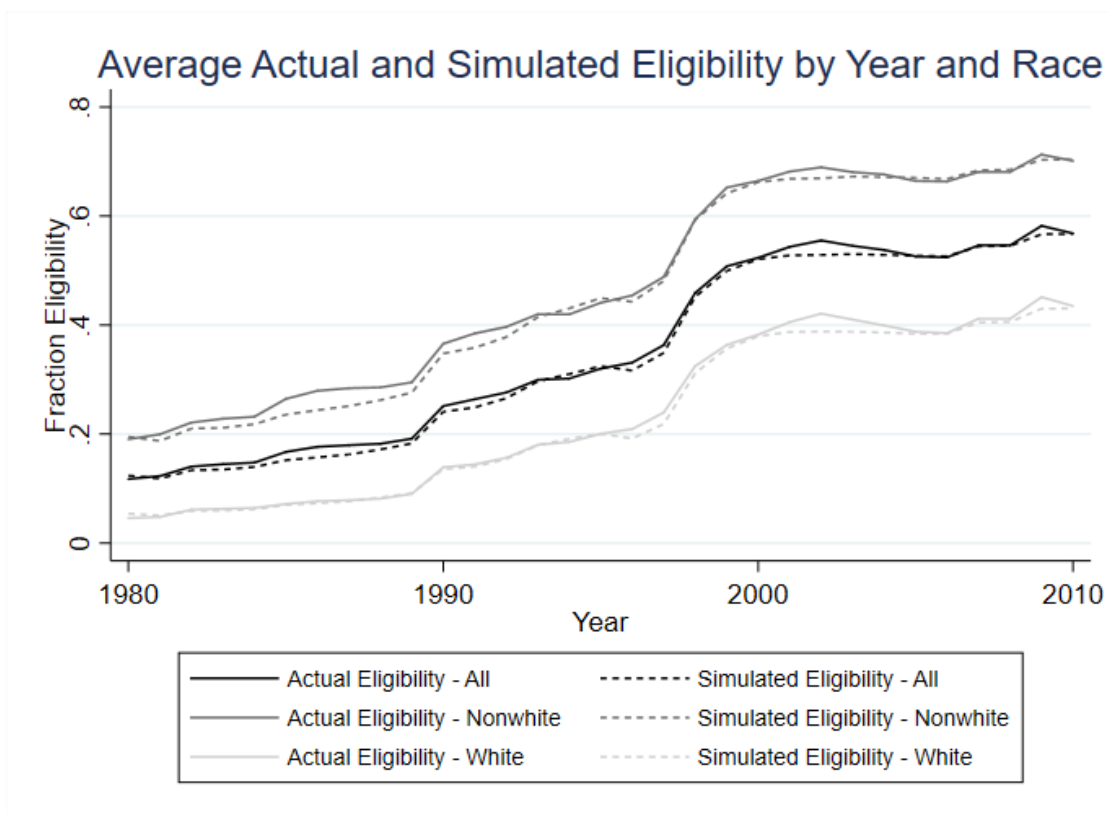


Figure A.2: Author's calculations using the 1979-2011 March CPS and Medicaid eligibility rules for each state and year. Each line shows average eligibility for children ages 0-17, by race. Simulated eligibility uses a fixed sample of the 1990 CPS. Incomes for the fixed sample are adjusted to each year using the CPI-U.

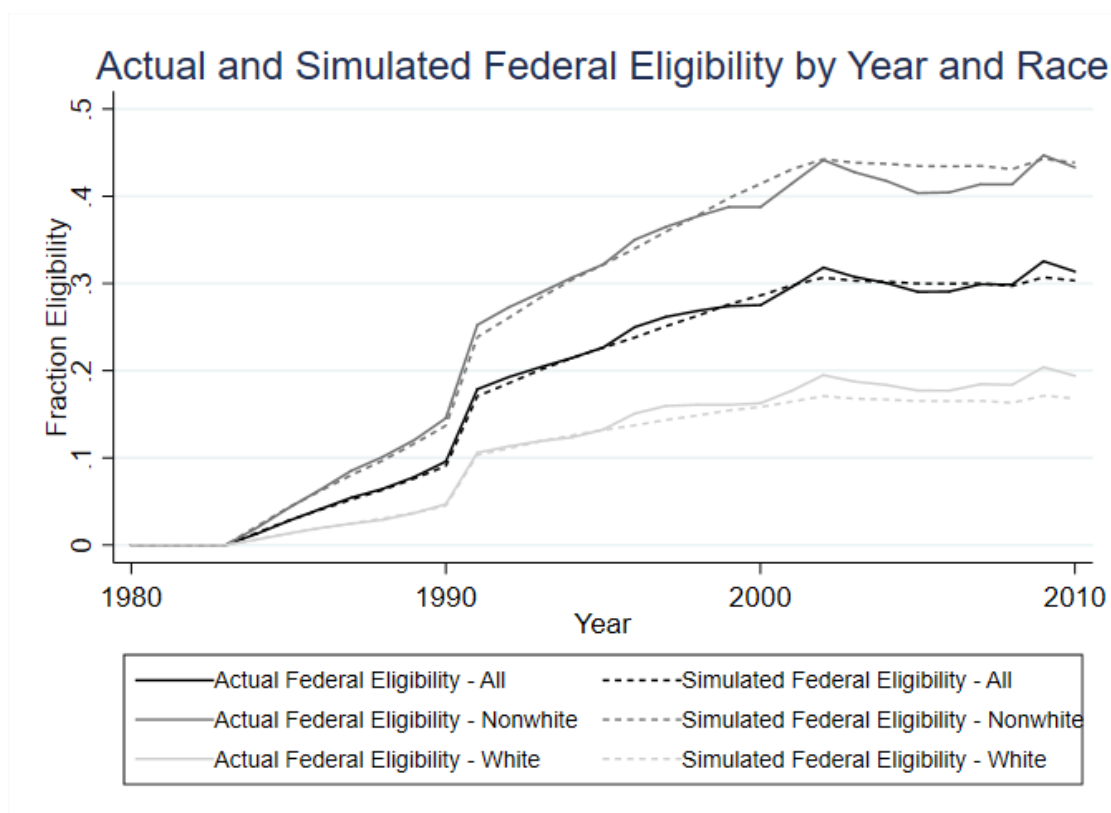


Figure A.3: Author's calculations using the 1979-2011 March CPS and Medicaid eligibility rules for each state and year. Each line shows average eligibility for children ages 0-17, by race. Simulated eligibility uses a fixed sample of the 1990 CPS. Incomes for the fixed sample are adjusted to each year using the CPI-U.

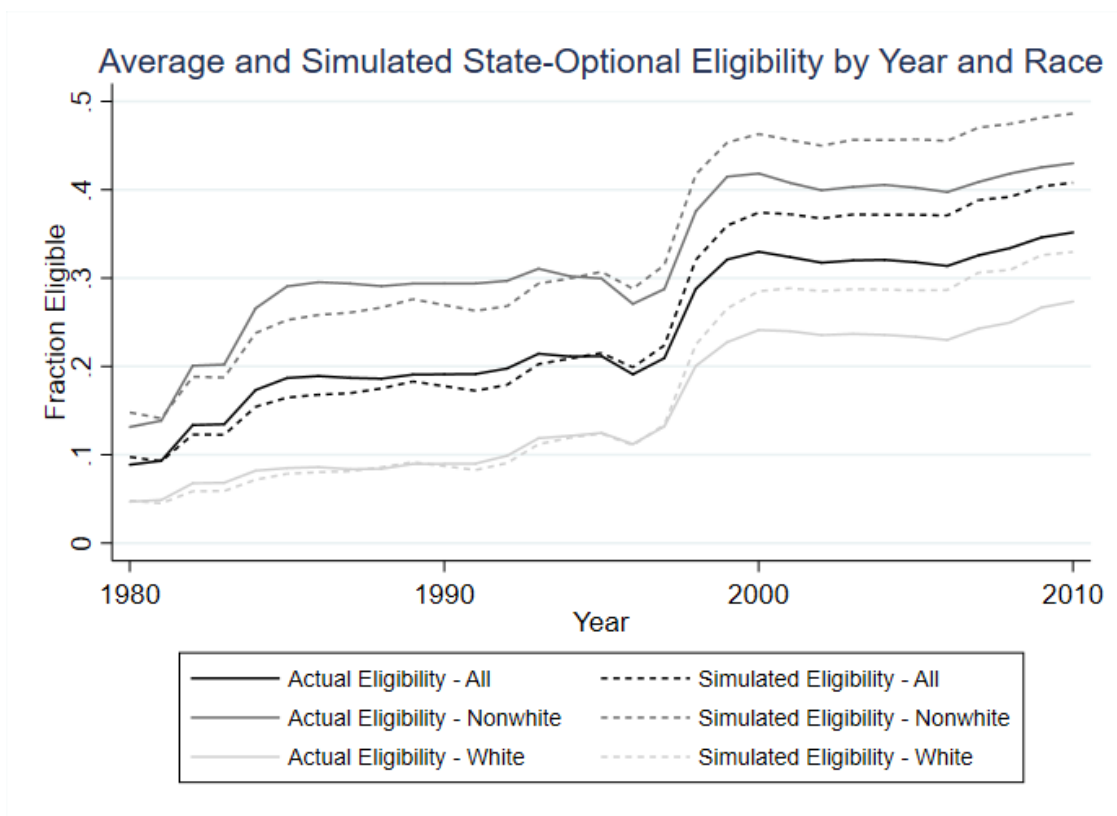


Figure A.4: Author's calculations using the 1979-2011 March CPS and Medicaid eligibility rules for each state and year. Each line shows average eligibility for children ages 0-17, by race. Simulated eligibility uses a fixed sample of the 1990 CPS. Incomes for the fixed sample are adjusted to each year using the CPI-U.

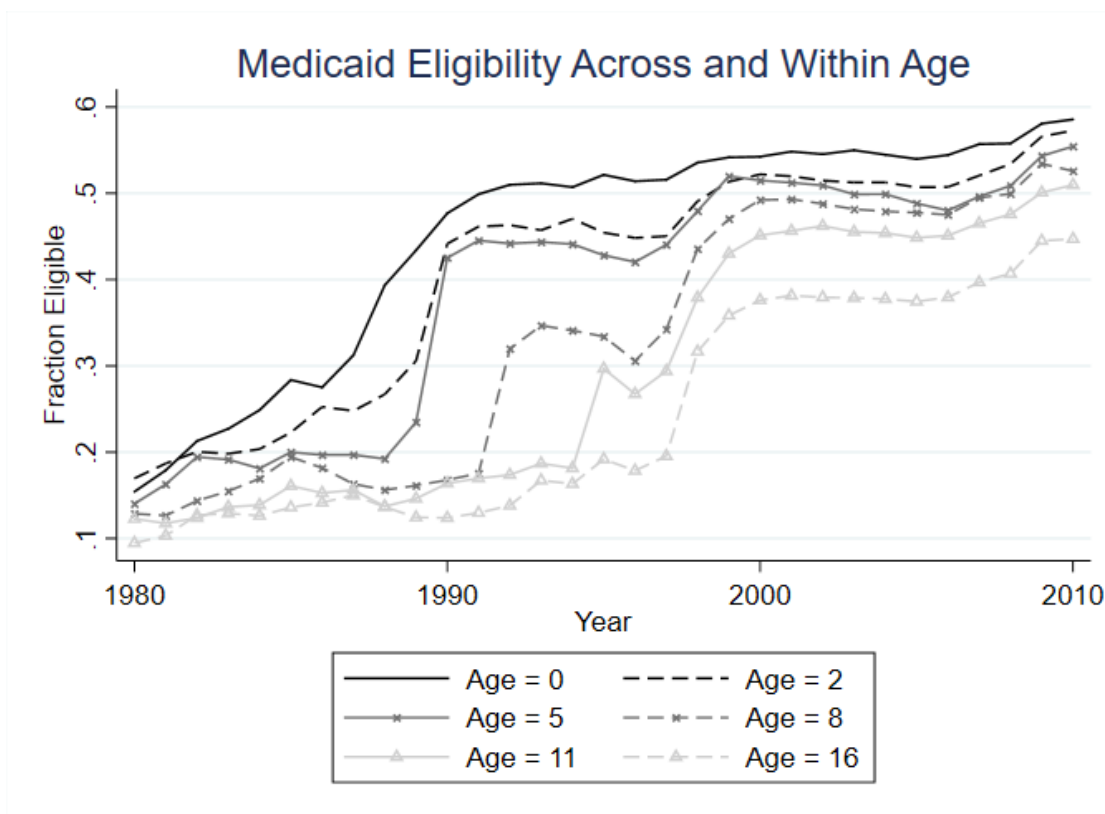


Figure A.5: Author's calculations using the 1979-2011 March CPS and Medicaid eligibility rules for each state and year. Each line shows average actual eligibility for children of a specific age. Only six ages are graphed to reduce clutter, but the omitted ages follow similar patterns.

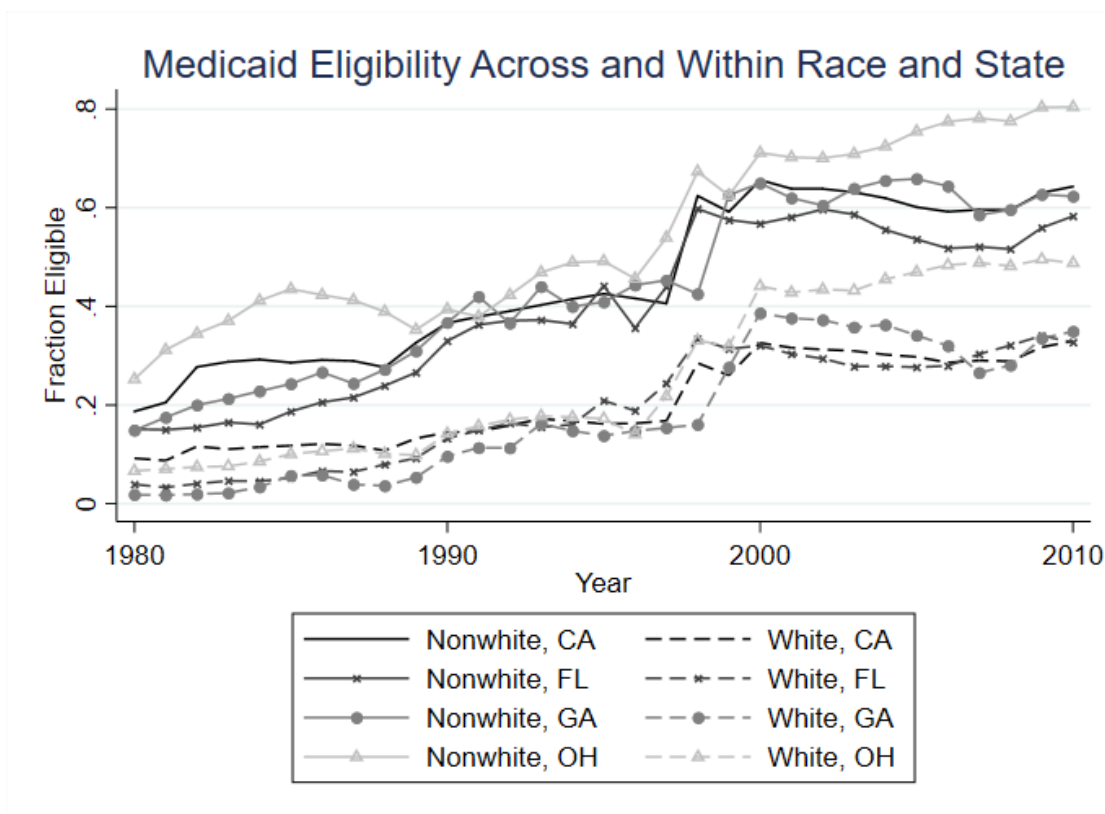


Figure A.6: Author's calculations using the 1979-2011 March CPS and Medicaid eligibility rules for each state and year. Each line shows average actual eligibility for children of a specific state and race.

Table A.1: Participation, Take-up, and Eligibility Rates for Major Safety Net Programs

	(1) SNAP	(2) Welfare	(3) AFDC	(4) TANF	(5) Housing	(6) EITC	(7) NSLP
Mean Participation Rate	0.137	0.111	0.154	0.045	0.056	-	0.455
Mean Take-up Rate	0.615	0.622	0.779	0.414	-	-	0.686
Mean Eligibility Rate	0.223	0.178	0.198	0.108	-	0.276	0.663

Estimates of participation, take-up, and eligibility for major safety net programs I consider. The CPS lacks sufficient information to estimate eligibility for rent subsidies, without which I am also unable to estimate take-up. The CPS measures eligibility for the EITC, and so I am unable to estimate participation and take-up. Participation rates are estimated directly from the CPS. For SNAP and cash welfare (AFDC/TANF), I simulate program rules using information on children's families' incomes, family structure, parental employment, state of residence, and number and ages of children.

Table A.2: The Effects of Average Overall Medicaid Eligibility on Program Participation, All Programs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
First Stage	SNAP	Welfare	AFDC	TANF	Housing	EITC	LIHEAP	SSI	SSDI	Part.	Part.
Panel A: OLS Results											
1. Single Fixed Effects (A,R,S,T)	-	0.177*** (0.024)	0.161*** (0.026)	0.342*** (0.030)	0.094*** (0.023)	0.118*** (0.023)	0.183*** (0.022)	0.009 (0.014)	0.045*** (0.014)	0.015*** (0.005)	-0.001 (0.004)
2. (1) + Preferred Paired FE (SR, RT, AR, AS)	-	0.164*** (0.014)	0.181*** (0.014)	0.207*** (0.025)	0.074*** (0.011)	0.079*** (0.013)	0.120*** (0.017)	-0.012 (0.009)	0.047*** (0.009)	0.006* (0.003)	0.007 (0.004)
Panel B: IV Results											
3. Single (A,R,S,T)	0.803*** (0.053)	-0.082*** (0.021)	-0.074*** (0.023)	0.098*** (0.030)	-0.066*** (0.023)	-0.004 (0.015)	0.023 (0.028)	-0.016 (0.021)	-0.052*** (0.011)	-0.014** (0.006)	-0.010 (0.007)
4. (3) + state-race FE (SR, A, T)	0.812*** (0.052)	-0.068*** (0.019)	-0.067*** (0.020)	0.078*** (0.014)	-0.015 (0.012)	0.004 (0.014)	0.025 (0.026)	-0.020 (0.020)	-0.044*** (0.011)	-0.009 (0.006)	-0.007 (0.007)
5. (4) + race-year FE (SR, RT, A)	0.802*** (0.050)	-0.011 (0.017)	0.046** (0.023)	0.093*** (0.017)	-0.003 (0.013)	0.013 (0.013)	-0.065*** (0.017)	-0.019 (0.020)	-0.015 (0.012)	-0.012** (0.006)	0.002 (0.006)
6. (5) + age-race FE (SR, RT, AR)	0.799*** (0.051)	-0.020 (0.017)	0.027 (0.022)	0.027* (0.016)	0.004 (0.013)	0.007 (0.013)	-0.076*** (0.017)	-0.050** (0.023)	-0.015 (0.011)	-0.003 (0.006)	0.005 (0.006)
7. Preferred Paired FE (SR, RT, AR, AS)	0.800*** (0.052)	-0.016 (0.017)	0.028 (0.023)	0.028* (0.016)	0.005 (0.013)	0.008 (0.013)	-0.071*** (0.017)	-0.044** (0.022)	-0.015 (0.011)	-0.002 (0.006)	0.006 (0.006)
8. (7) + age-year FE (SR, RT, AR, AS, AT)	0.740*** (0.059)	-0.044** (0.022)	0.014 (0.027)	0.026 (0.022)	-0.000 (0.014)	-0.002 (0.015)	-0.085*** (0.022)	-0.011 (0.029)	-0.019 (0.014)	-0.006 (0.008)	-0.011 (0.008)
9. (7) + state-year (SR, RT, AR, AS, ST)	0.935*** (0.049)	0.034** (0.014)	0.077*** (0.019)	0.021 (0.014)	0.049 (0.031)	0.014 (0.010)	-0.092*** (0.017)	-0.085*** (0.016)	-0.015 (0.012)	-0.010 (0.007)	0.027*** (0.007)
10. Saturated 2-Way FE (SR, RT, AR, AS, AT, ST)	0.852*** (0.073)	0.012 (0.032)	0.114*** (0.039)	-0.002 (0.019)	0.035 (0.044)	-0.020 (0.018)	-0.181*** (0.033)	-0.008 (0.026)	-0.033 (0.025)	-0.040*** (0.014)	-0.014 (0.012)
Mean Dep. Var.	0.294	0.137	0.111	0.154	0.045	0.056	0.276	0.455	0.053	0.027	0.048

Each cell is a regression of a program participation outcome on average overall Medicaid eligibility with the exception of Column 7, which measures EITC eligibility. Rows 1 and 2 estimate OLS specifications using single fixed effects defining cells in Row 1 (age, state, race, and year), and the preferred specification of two-way fixed effects in Row 2 (same as Row 7 and used in Table 1.2). Rows 3 repeats the single fixed effects specification and instruments Medicaid eligibility using simulated eligibility. Rows 4 to 7 iteratively add two-way fixed effects for state-race, race-year, age-race, and age-state, where Row 7 uses the preferred instrumental variables specification from Equation 1.1. Rows 8 and 9 add age-year and state-year fixed effects separately to the specification from Row 7, and Row 10 includes all two-way fixed effects, although including both age-year and state-year fixed effects eliminates the primary sources of identifying variation for Equation 1.1. All regressions control for state unemployment rates, number of children in the family, and simulated eligibility for the relevant outcome program for SNAP, cash Welfare, the EITC, the NSLP, and LIHEAP. Each regression has at most 53,098 observations. Standard errors are clustered at the level of the state. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table A.3: The Effects of Average Federal Medicaid Eligibility on Program Participation, All Programs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
First Stage		SNAP	Welfare	AFDC	TANF	Housing	EITC	NSLP	LIHEAP	SSI	SSDI
Panel A: OLS Results											
1. Single Fixed Effects (A,R,S,T)	-	0.139*** (0.014)	0.081*** (0.012)	0.127*** (0.015)	0.162*** (0.022)	0.083*** (0.012)	0.152*** (0.015)	0.034*** (0.013)	0.009 (0.011)	0.012** (0.004)	-0.003 (0.004)
2. (1) + Preferred Paired FE (SR, RT, AR, AS)	-	0.138*** (0.014)	0.122*** (0.014)	0.067*** (0.015)	0.128*** (0.012)	0.069*** (0.011)	0.081*** (0.015)	-0.009 (0.008)	0.030*** (0.008)	0.015*** (0.004)	0.006 (0.004)
Panel B: IV Results											
3. Single (A,R,S,T)	0.942*** (0.021)	-0.028 (0.017)	-0.080*** (0.022)	0.061*** (0.014)	0.092*** (0.016)	-0.001 (0.010)	0.118*** (0.014)	0.066*** (0.021)	-0.062*** (0.015)	-0.015*** (0.005)	-0.011* (0.006)
4. (3) + state-race FE (SR, A, T)	0.949*** (0.022)	-0.017 (0.016)	-0.069*** (0.019)	0.062*** (0.011)	0.094*** (0.015)	0.006 (0.009)	0.109*** (0.015)	0.068*** (0.019)	-0.060*** (0.016)	-0.012** (0.005)	-0.009 (0.006)
5. (4) + race-year FE (SR, RT, A)	1.024*** (0.023)	0.060*** (0.007)	0.094*** (0.014)	0.072*** (0.013)	0.124*** (0.017)	0.030*** (0.009)	0.011 (0.012)	0.075*** (0.014)	-0.005 (0.008)	-0.020*** (0.004)	-0.002 (0.005)
6. (5) + age-race FE (SR, RT, AR)	1.041*** (0.023)	0.034*** (0.009)	0.054*** (0.013)	0.009 (0.013)	0.072*** (0.013)	0.011** (0.005)	-0.021** (0.009)	-0.000 (0.010)	-0.004 (0.006)	0.002 (0.004)	0.006 (0.004)
7. Preferred Paired FE (SR, RT, AR, AS)	1.046*** (0.022)	0.032*** (0.009)	0.051*** (0.012)	0.007 (0.011)	0.074*** (0.014)	0.017*** (0.006)	-0.002 (0.009)	-0.017* (0.009)	-0.005 (0.006)	0.002 (0.004)	0.004 (0.004)
8. (7) + age-year FE (SR, RT, AR, AS, AT)	1.051*** (0.053)	0.029 (0.019)	0.059** (0.030)	-0.012 (0.018)	0.064* (0.037)	0.002 (0.010)	-0.033* (0.018)	-0.025 (0.018)	-0.011 (0.017)	-0.012 (0.008)	-0.011 (0.008)
9. (7) + state-year (SR, RT, AR, AS, ST)	1.043*** (0.023)	0.033*** (0.008)	0.051*** (0.011)	0.007 (0.011)	0.072*** (0.014)	0.016*** (0.006)	-0.012 (0.009)	-0.010 (0.009)	-0.003 (0.005)	0.003 (0.004)	0.005 (0.004)
10. Saturated 2-Way FE (SR, RT, AR, AS, AT, ST)	1.038*** (0.054)	0.023 (0.015)	0.047** (0.023)	-0.018 (0.016)	0.061* (0.036)	0.006 (0.010)	-0.020 (0.018)	0.006 (0.019)	-0.001 (0.011)	-0.010 (0.008)	-0.008 (0.008)
Mean Dep. Var.	0.169	0.137	0.111	0.154	0.045	0.056	0.276	0.455	0.053	0.027	0.048

Each cell is a regression of a program participation outcome on average federal Medicaid eligibility with the exception of Column 7, which measures EITC eligibility. Rows 1 and 2 estimate OLS specifications using single fixed effects defining cells in Row 1 (age, state, race, and year), and the preferred specification of two-way fixed effects in Row 2 (same as Row 7 and used in Table 1.2). Rows 3 repeats the single fixed effects specification and instruments Medicaid eligibility using simulated eligibility. Rows 4 to 7 iteratively add two-way fixed effects for state-race, race-year, age-race, and age-state, where Row 7 uses the preferred instrumental variables specification from Equation 1.1. Rows 8 and 9 add age-year and state-year fixed effects separately to the specification from Row 7, and Row 10 includes all two-way fixed effects, although including both age-year and state-year fixed effects eliminates the primary sources of identifying variation for Equation 1.1. All regressions control for state unemployment rates, number of children in the family, and simulated eligibility for the relevant outcome program for SNAP, cash Welfare, the EITC, the NSLP, and LIHEAP. Each regression has at most 53,098 observations. Standard errors are clustered at the level of the state. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively

Table A.4: The Effects of Average State-Optional Medicaid Eligibility Program Participation, on All Programs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	First Stage	SNAP	Welfare	AFDC	TANF	Housing	EITC	NSLP	LIHEAP	SSI	SSDI
Panel A: OLS Results											
1. Single Fixed Effects (A,R,S,T)	-	0.142*** (0.031)	0.235*** (0.032)	0.372*** (0.022)	0.084*** (0.025)	0.102*** (0.026)	0.044 (0.029)	0.020 (0.018)	0.064*** (0.014)	0.003 (0.008)	-0.017*** (0.005)
2. (1) + Preferred Paired FE (SR, RT, AR, AS)	-	0.121*** (0.014)	0.186*** (0.015)	0.238*** (0.025)	0.051*** (0.012)	0.063*** (0.011)	0.044*** (0.014)	-0.008 (0.011)	0.052*** (0.008)	0.002 (0.005)	-0.014*** (0.004)
Panel B: IV Results											
3. Single (A,R,S,T)	0.834*** (0.034)	-0.049 (0.035)	0.049 (0.031)	0.100*** (0.038)	-0.010 (0.019)	0.003 (0.023)	-0.038 (0.031)	0.031 (0.029)	-0.004 (0.013)	-0.014 (0.010)	-0.019*** (0.006)
4. (3) + state-race FE (SR, A, T)	0.833*** (0.033)	-0.020 (0.017)	0.056** (0.026)	0.084*** (0.014)	0.002 (0.010)	0.016 (0.011)	-0.036 (0.027)	0.002 (0.017)	0.011 (0.011)	-0.005 (0.005)	-0.012** (0.005)
5. (4) + race-year FE (SR, RT, A)	0.826*** (0.034)	-0.008 (0.016)	0.066** (0.027)	0.102*** (0.016)	0.000 (0.010)	0.015 (0.012)	-0.048** (0.019)	0.008 (0.018)	0.008 (0.013)	-0.004 (0.005)	-0.010* (0.006)
6. (5) + age-race FE (SR, RT, AR)	0.835*** (0.036)	-0.010 (0.016)	0.050* (0.027)	0.022* (0.012)	-0.001 (0.010)	0.014 (0.013)	-0.050*** (0.018)	-0.022 (0.019)	0.010 (0.013)	0.005 (0.005)	-0.007 (0.005)
7. Preferred Paired FE (SR, RT, AR, AS)	0.829*** (0.037)	-0.013 (0.016)	0.048* (0.028)	0.016 (0.013)	-0.007 (0.013)	0.013 (0.013)	-0.049*** (0.019)	-0.015 (0.021)	0.010 (0.014)	0.005 (0.006)	-0.008 (0.005)
8. (7) + age-year FE (SR, RT, AR, AS, AT)	0.792*** (0.041)	-0.031 (0.019)	0.025 (0.032)	0.016 (0.016)	0.001 (0.014)	0.011 (0.016)	-0.053*** (0.020)	0.023 (0.027)	0.007 (0.017)	0.002 (0.006)	-0.002 (0.006)
9. (7) + state-year (SR, RT, AR, AS, ST)	0.974*** (0.027)	0.042*** (0.016)	0.132*** (0.013)	-0.000 (0.012)	-0.021 (0.024)	0.014 (0.010)	-0.077*** (0.020)	-0.075*** (0.020)	0.015 (0.010)	-0.001 (0.008)	-0.017** (0.007)
10. Saturated 2-Way FE (SR, RT, AR, AS, AT, ST)	0.911*** (0.037)	0.017 (0.023)	0.114*** (0.022)	-0.010 (0.018)	0.015 (0.031)	0.008 (0.017)	-0.118*** (0.025)	0.032 (0.027)	0.004 (0.015)	-0.017 (0.011)	-0.003 (0.009)
Mean Dep. Var.	0.218	0.137	0.111	0.154	0.045	0.056	0.276	0.455	0.053	0.027	0.048

Each cell is a regression of a program participation outcome on average state-optional Medicaid eligibility with the exception of Column 7, which measures EITC eligibility. Rows 1 and 2 estimate OLS specifications using single fixed effects defining cells in Row 1 (age, state, race, and year), and the preferred specification of two-way fixed effects in Row 2 (same as Row 7 and used in Table 1.2). Rows 3 repeats the single fixed effects specification and instruments Medicaid eligibility using simulated eligibility. Rows 4 to 7 iteratively add two-way fixed effects for state-race, race-year, age-race, and age-state, where Row 7 uses the preferred instrumental variables specification from Equation 1.1. Rows 8 and 9 add age-year and state-year fixed effects separately to the specification from Row 7, and Row 10 includes all two-way fixed effects, although including both age-year and state-year fixed effects eliminates the primary sources of identifying variation for Equation 1.1. All regressions control for state unemployment rates, number of children in the family, and simulated eligibility for the relevant outcome program for SNAP, cash Welfare, the EITC, the NSLP, and LIHEAP. Each regression has at most 53,098 observations. Standard errors are clustered at the level of the state. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively

Table A.5: The Effects of Average Overall Medicaid Eligibility on Labor Supply

	(1) First Stage	(2) Total Family Hours	(3) Total Adult Hours	(4) Total Teen Hours	(5) Total Adult Labor Income	(6) Total Teen Labor Income	(7) Total Family Labor Income
Panel A: OLS Results							
1. Single Fixed Effects (A,R,S,T)	-	-13.628*** (1.975)	-15.452*** (1.846)	1.824*** (0.347)	-44472.72*** (5498.36)	-44886.78*** (5477.91)	414.06*** (85.61)
2. (1) + Preferred Paired FE (SR, RT, AR, AS)	-	-14.838*** (1.466)	-15.040*** (1.381)	0.201 (0.241)	-28099.65*** (2757.00)	-28039.79*** (2734.14)	-59.86 (65.91)
Panel B: IV Results							
3. Single (A,R,S,T)	0.803*** (0.053)	12.832*** (2.442)	7.209*** (1.841)	5.623*** (0.714)	8152.66 (8128.75)	7217.68 (8163.73)	934.98*** (144.09)
4. (3) + state-race FE (SR, A, T)	0.812*** (0.052)	11.524*** (2.177)	5.981*** (1.582)	5.543*** (0.696)	5427.94 (6994.52)	4508.96 (7034.10)	918.99*** (140.40)
5. (4) + race-year FE (SR, RT, A)	0.802*** (0.050)	9.156*** (1.823)	3.720*** (1.333)	5.436*** (0.718)	21148.99*** (7187.60)	20305.05*** (7219.74)	843.94*** (147.64)
6. (5) + age-race FE (SR, RT, AR)	0.799*** (0.051)	7.248*** (1.731)	3.479*** (1.353)	3.769*** (0.532)	21100.58*** (7545.93)	20643.63*** (7579.32)	456.95*** (125.53)
7. Preferred Paired FE (SR, RT, AR, AS)	0.800*** (0.052)	7.521*** (1.761)	3.810*** (1.398)	3.710*** (0.503)	21225.59*** (7687.05)	20758.53*** (7719.65)	467.06*** (125.88)
8. (7) + age-year FE (SR, RT, AR, AS, AT)	0.740*** (0.059)	4.229** (2.040)	2.331 (1.670)	1.899*** (0.501)	26502.97** (10836.51)	26185.62** (10899.75)	317.35** (151.75)
9. (7) + state-year (SR, RT, AR, AS, ST)	0.935*** (0.049)	9.513*** (1.964)	2.992** (1.336)	6.521*** (0.919)	10789.61*** (2796.70)	9935.22*** (2705.84)	854.38*** (196.85)
10. Saturated 2-Way FE (SR, RT, AR, AS, AT, ST)	0.852*** (0.073)	-2.537 (2.400)	-5.389*** (2.073)	2.852*** (0.918)	13161.06** (5525.88)	12369.22** (5481.18)	791.84*** (266.08)
Mean Dep. Var.	0.294	59.355	55.312	4.042	59651.02	58857.35	793.67

Each cell is a regression of a labor supply outcome on average overall Medicaid eligibility. Columns 2-4 estimate the effect of overall Medicaid eligibility on total usual weekly hours worked by families, adults, and teens, respectively. Columns 5-7 estimate the effect on total annual income for families, adults, and teens, respectively. Rows 1 and 2 estimate OLS specifications using single fixed effects defining cells in Row 1 (age, state, race, and year), and the preferred specification of two-way fixed effects in Row 2 (same as Row 1 and used in Table 1.2). Rows 3 repeats the single fixed effects specification and instruments Medicaid eligibility using simulated eligibility. Rows 4 to 7 iteratively add two-way fixed effects for state-race, race-year, age-race, and age-state, where Row 7 uses the preferred instrumental variables specification from Equation 1.1. Rows 8 and 9 add age-year and state-year fixed effects separately to the specification from Row 7, and Row 10 includes all two-way fixed effects, although including both age-year and state-year fixed effects eliminates the primary sources of identifying variation for Equation 1.1. All regressions control for state unemployment rates, number of children in the family. Each regression has at most 53,098 observations. Standard errors are clustered at the level of the state. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table A.6: The Effects of Average Federal Medicaid Eligibility on Labor Supply

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
First Stage	Total Family	Total Adult	Total Teen	Total Adult	Total Teen	Total Family	Total Family
	Hours	Hours	Hours	Hours	Hours	Hours	Hours
Panel A: OLS Results							
1. Single Fixed Effects (A,R,S,T)	-	-4.858*** (1.374)	-7.388*** (1.250)	2.530*** (0.337)	-26316.11*** (3564.64)	-26988.41*** (3563.39)	672.30*** (66.20)
2. (1) + Preferred Paired FE (SR, RT, AR, AS)	-	-11.633*** (1.586)	-10.757*** (1.472)	-0.876*** (0.262)	-19784.89*** (2703.35)	-19671.14*** (2686.04)	-113.75 (86.49)
Panel B: IV Results							
3. Single (A,R,S,T)	0.942*** (0.021)	11.718*** (1.780)	7.053*** (1.383)	4.664*** (0.509)	-13083.29*** (2124.34)	-14012.02*** (2101.65)	928.73*** (104.56)
4. (3) + state-race FE (SR, A, T)	0.949*** (0.022)	10.671*** (1.558)	6.088*** (1.199)	4.583*** (0.493)	-13590.51*** (1794.08)	-14501.22*** (1780.02)	910.71*** (103.16)
5. (4) + race-year FE (SR, RT, A)	1.024*** (0.023)	6.715*** (0.929)	1.769** (0.743)	4.946*** (0.430)	2690.74** (1065.81)	1701.10 (1082.71)	989.64*** (106.01)
6. (5) + age-race FE (SR, RT, AR)	1.041*** (0.023)	0.146 (0.850)	0.009 (0.774)	0.137 (0.296)	881.19 (1025.40)	929.86 (1015.92)	-48.67 (121.12)
7. Preferred Paired FE (SR, RT, AR, AS)	1.046*** (0.022)	0.179 (0.760)	0.195 (0.724)	-0.016 (0.279)	498.042 (1014.40)	580.111 (1021.38)	-82.069 (108.51)
8. (7) + age-year FE (SR, RT, AR, AS, AT)	1.051*** (0.053)	-1.055 (1.984)	-1.818 (1.736)	0.762 (0.578)	-3918.41* (2029.40)	-4271.02** (1973.10)	352.61** (178.84)
9. (7) + state-year (SR, RT, AR, AS, ST)	1.043*** (0.023)	0.508 (0.765)	0.521 (0.731)	-0.014 (0.280)	933.294 (1025.04)	1028.601 (1030.14)	-95.307 (106.99)
10. Saturated 2-Way FE (SR, RT, AR, AS, AT, ST)	1.038*** (0.054)	0.126 (1.809)	-0.617 (1.625)	0.743 (0.543)	-3627.206 (2264.03)	-3950.214* (2257.90)	323.009* (186.73)
Mean Dep. Var.	0.169	59.355	55.312	4.042	59651.02	58857.35	793.67

Each cell is a regression of a labor supply outcome on average federal Medicaid eligibility. Columns 2-4 estimate the effect of overall Medicaid eligibility on total usual weekly hours worked by families, adults, and teens, respectively. Columns 5-7 estimate the effect on total annual income for families, adults, and teens, respectively. Rows 1 and 2 estimate OLS specifications using single fixed effects defining cells in Row 1 (age, state, race, and year), and the preferred specification of two-way fixed effects in Row 2 (same as Row 1 and used in Table 1.2). Rows 3 repeats the single fixed effects specification and instruments Medicaid eligibility using simulated eligibility. Rows 4 to 7 iteratively add two-way fixed effects for state-race, race-year, age-race, and age-state, where Row 7 uses the preferred instrumental variables specification from Equation 1.1. Rows 8 and 9 add age-year and state-year fixed effects separately to the specification from Row 7, and Row 10 includes all two-way fixed effects, although including both age-year and state-year fixed effects eliminates the primary sources of identifying variation for Equation 1.1. All regressions control for state unemployment rates, number of children in the family. Each regression has at most 53,098 observations. Standard errors are clustered at the level of the state. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table A.7: The Effects of Average State-Optional Medicaid Eligibility on Labor Supply

	(1) First Stage	(2) Total Family Hours	(3) Total Adult Hours	(4) Total Teen Hours	(5) Total Adult Labor Income	(6) Total Teen Labor Income	(7) Total Family Labor Income
Panel A: OLS Results							
1. Single Fixed Effects (A,R,S,T)	-	-16.758*** (2.706)	-16.269*** (2.583)	-0.489 (0.304)	-28465.32*** (5171.70)	-28616.13*** (5159.83)	150.82* (78.04)
2. (1) + Preferred Paired FE (SR, RT, AR, AS)	-	-15.997*** (1.281)	-14.110*** (1.163)	-1.887*** (0.326)	-19919.41*** (2242.73)	-19766.99*** (2211.80)	-152.42** (72.30)
Panel B: IV Results							
3. Single (A,R,S,T)	0.834*** (0.034)	4.801 (3.272)	2.400 (2.988)	2.401*** (0.459)	14403.00** (6972.05)	13883.25** (7014.96)	519.75*** (99.51)
4. (3) + state-race FE (SR, A, T)	0.833*** (0.033)	1.920 (1.679)	-0.348 (1.408)	2.268*** (0.439)	13539.44** (6190.53)	13054.09** (6226.58)	485.35*** (109.58)
5. (4) + race-year FE (SR, RT, A)	0.826*** (0.034)	2.079 (1.508)	-0.129 (1.277)	2.208*** (0.430)	15831.47*** (6020.85)	15384.34** (6066.65)	447.13*** (119.17)
6. (5) + age-race FE (SR, RT, AR)	0.835*** (0.036)	1.140 (1.540)	0.176 (1.334)	0.963** (0.382)	15758.31** (6367.74)	15583.47** (6414.71)	174.84 (121.65)
7. Preferred Paired FE (SR, RT, AR, AS)	0.829*** (0.037)	1.394 (1.603)	0.586 (1.411)	0.808* (0.416)	16828.22** (6747.85)	16668.38** (6795.75)	159.84 (127.01)
8. (7) + age-year FE (SR, RT, AR, AS, AT)	0.792*** (0.041)	5.055** (2.062)	3.072* (1.755)	1.983*** (0.449)	21756.96*** (8331.57)	21491.25** (8387.04)	265.72** (126.16)
9. (7) + state-year (SR, RT, AR, AS, ST)	0.974*** (0.027)	-7.440*** (1.225)	-6.950*** (1.084)	-0.489 (0.642)	2371.80 (2229.97)	2112.73 (2211.20)	259.08** (128.82)
10. Saturated 2-Way FE (SR, RT, AR, AS, AT, ST)	0.911*** (0.037)	-1.333 (1.373)	-3.912*** (1.345)	2.579*** (0.555)	9469.55** (3824.09)	8762.31** (3826.26)	707.24*** (152.38)
Mean Dep. Var.	0.218	59.355	55.312	4.042	59651.02	58857.35	793.67

Each cell is a regression of a labor supply outcome on average state-optional Medicaid eligibility. Columns 2-4 estimate the effect of overall Medicaid eligibility on total usual weekly hours worked by families, adults, and teens, respectively. Columns 5-7 estimate the effect on total annual income for families, adults, and teens, respectively. Rows 1 and 2 estimate OLS specifications using single fixed effects defining cells in Row 1 (age, state, race, and year), and the preferred specification of two-way fixed effects in Row 2 (same as Row 7 and used in Table 1.2). Rows 3 repeats the single fixed effects specification and instruments Medicaid eligibility using simulated eligibility. Rows 4 to 7 iteratively add two-way fixed effects for state-race, race-year, age-race, and age-state, where Row 7 uses the preferred instrumental variables specification from Equation 1.1. Rows 8 and 9 add age-year and state-year fixed effects separately to the specification from Row 7, and Row 10 includes all two-way fixed effects, although including both age-year and state-year fixed effects eliminates the primary sources of identifying variation for Equation 1.1. All regressions control for state unemployment rates, number of children in the family. Each regression has at most 53,098 observations. Standard errors are clustered at the level of the state. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively

Table A.8: The Effects of Medicaid Eligibility on Simulated Program Eligibility

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	SNAP	Welfare	AFDC	TANF	EITC	NSLP	LIHEAP
Panel A: Simulated Eligibility as Outcome							
1. Overall Eligibility	0.012*** (0.005)	-0.006 (0.014)	0.025*** (0.006)	0.003 (0.009)	-0.052 (0.033)	-0.014 (0.013)	0.007 (0.007)
2. Federal Eligibility	0.007 (0.009)	-0.015 (0.012)	0.001 (0.002)	0.006 (0.004)	-0.022 (0.029)	0.005 (0.008)	-0.011 (0.010)
Panel B: Program Participation as Outcome							
3. Overall Eligibility	-0.024 (0.019)	0.028 (0.022)	0.019 (0.014)	0.005 (0.013)	-0.084*** (0.017)	-0.054*** (0.025)	-0.018 (0.011)
4. Federal Eligibility	0.035*** (0.008)	0.055*** (0.012)	0.006 (0.010)	0.070*** (0.018)	-0.008 (0.009)	-0.016 (0.010)	-0.001 (0.007)

Each cell is a regression of a different program participation outcome on average Medicaid eligibility, where every regression uses the preferred IV specification including state-race, race-year, age-race, and age-state fixed effects. Panel A regresses fixed, simulated eligibility for outcome programs on Medicaid eligibility, and thus measures the degree to which outcome program eligibility rules are changing in ways that are correlated with Medicaid eligibility. These correlations are significant between SNAP and AFDC rules and overall Medicaid expansions, but no programs have eligibility rules that are significantly correlated with federal medicaid expansions. These simulated outcome program eligibility measures are included as controls for estimates in Tables 1.2, 1.3, 1.7, 1.8, and Appendix Tables A.2-A.4 and A.9. Panel B estimates the effects on program participation when excluding these controls. When compared to the results from Table 1.2, even the correlations between SNAP and AFDC and Medicaid expansions do not affect the qualitative conclusions. All regressions control for state unemployment rates, number of children in the family, and simulated eligibility for the relevant outcome program. Alternative fixed effects specifications are available in the Appendix. Each regression has at most 53,098 observations. Standard errors are clustered at the level of the state. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively

Table A.9: The Effects of Medicaid Expansions on Program Participation of Families Without Children

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	First Stage	SNAP	Welfare	AFDC	TANF	Housing	EITC	NSLP	LIHEAP	SSI	SSDI
		Part.	Part.	Part.	Part.	Part.	Eligibility	Part.	Part.	Part.	Part.
1. Overall Eligibility	0.757*** (0.051)	-0.002 (0.003)	0.003 (0.003)	0.004 (0.003)	0.002 (0.002)	0.005** (0.002)	-0.020** (0.010)	0.000 (0.001)	0.005* (0.003)	-0.000 (0.002)	-0.001 (0.002)
2. Federal Eligibility	1.020*** (0.025)	-0.001 (0.003)	0.002 (0.002)	0.001 (0.003)	0.006* (0.004)	-0.002 (0.003)	-0.010 (0.006)	0.001 (0.001)	0.003 (0.003)	-0.001 (0.003)	-0.003 (0.002)
Mean Dep. Var.	-	0.042	0.008	0.011	0.004	0.022	0.046	0.004	0.014	0.015	0.015

Each cell is a regression of a program participation outcome on average overall Medicaid eligibility with the exception of Column 7, which measures EITC eligibility. Row 1 uses variation from overall medicaid expansions. Row 2 uses variation only from federal expansions. This table uses the CPS sample of families without children, but randomly assigns children age 0-17 to these households from a uniform age distribution. All specifications use the preferred specification of two-way fixed effects from Equation 1.1. All regressions control for state unemployment rates, number of children in the family, and simulated eligibility for the relevant outcome program for SNAP, cash Welfare, the EITC, the NSLP, and LIHEAP. Each regression has at most 53,098 observations. Standard errors are clustered at the level of the state. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively

Table A.10: The Effects of Medicaid Expansions on Labor Supply of Families Without Children

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Single Mother Hours	Single Mother Labor Income	Single Parent Hours	Single Parent Labor Income	Two Parent Hours	Two Parent Labor Income	Other Adults Hours	Other Adults Labor Income
1. Overall Eligibility	-0.097 (0.470)	1242.16 (942.95)	-0.017 (0.905)	2591.70 (1836.92)	0.714 (0.844)	1205.48* (703.95)	-0.818* (0.426)	-55.38 (220.94)
2. Federal Eligibility	-0.323 (0.424)	473.41 (567.47)	-0.405 (0.660)	1224.27 (985.32)	-0.029 (0.712)	557.08 (524.43)	0.118 (0.347)	55.72 (217.48)
Mean Dep. Var.	5.622	3249.93	14.101	8441.63	11.531	7940.95	4.906	1901.39

Each cell is a regression of a labor supply outcome on average overall Medicaid eligibility. Row 1 uses variation from overall medicaid expansions. Row 2 uses variation only from federal expansions. This table uses the CPS sample of families without children, but randomly assigns children age 0-17 to these households from a uniform age distribution. All specifications use the preferred specification of two-way fixed effects from Equation 1.1. All regressions control for state unemployment rates, number of children in the family. Each regression has at most 53,098 observations. Standard errors are clustered at the level of the state. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively

APPENDIX B
CHAPTER 2 APPENDIX

Some questions in the AHEAD/HRS are not consistent between years, particularly the questions asked regarding health expenditures. The questions for each of the years in my sample are listed below for the various years of the survey. The 1993 survey had a single question asking broadly about health care expenses over the last 12 months:

- Not counting costs covered by insurance, about how much did you [and your (husband/wife/partner)] end up paying for any part of hospital and doctor bills and any other medical or dental expenses in the last 12 months, since MONTH of (1992/1993)?

Beginning in 1995, that single question is split into four separate questions regarding different categories of medical expenses over a period of two years rather than the last 12 months. Three ask for the total spending out-of-pocket for a category over that time period, while a single asks for the out-of-pocket amount per month over that time period:

- About how much did you pay out-of-pocket for (nursing home/hospital) bills (since (W1 Interview Month-Year)/in the last two years)?
- About how much did you pay out-of-pocket for (doctor/outpatient surgery/dental) bills (since (W1 Interview Month-Year)/in the last two years)?

- On the average, about how much have you paid out-of-pocket per month for these prescriptions (since (W1 Interview Month-Year)/in the last two years)?
- About how much did you pay out-of-pocket for (in-home medical care/special facilities or services) (since (W1 Interview Month-year)/in the last two years)?

The questions for 1998 and 2000 are nearly identical to those from 1995:

- About how much did you pay out-of-pocket for (nursing home/hospital/nursing home and hospital/...) bills (since Q218-PREV WAVE IW MONTH / Q219-PREV WAV IW YEAR/in the last two year)?
- About how much did you pay out-of-pocket for (doctor/outpatient/surgery /dental/doctor and outpatient surgery/doctor and dental/outpatient surgery and dental/doctor, outpatient surgery, and dental/...) bills (since Q218-PREV WAVE IW MONTH / Q219-PREV WAVE IW YEAR/in the last two years)?
- On the average, about how much have you paid out-of-pocket per month for these prescriptions (since Q218-PREV WAVE IW MONTH / Q219-PREV WAVE IW YEAR/in the last two years)?
- About how much did you pay out-of-pocket for (in-home medical care/special facilities or services/in-home medical care, special facilities or services/...) (since Q218-PREV WAVE IW MONTH / Q219-PREV WAVE IW YEAR/in the last two years)?

I combine these questions into a single aggregate out-of-pocket spending variable for each household for the last 12 months. Thus, the answers to the

question for 1993 are unchanged. For answers from the 1995, 1998, and 2000 surveys I multiply the answer for the prescriptions question by 12 to obtain a yearly value, divide the answers to the other questions by two, and then sum the four to obtain a single variable, as with 1993.

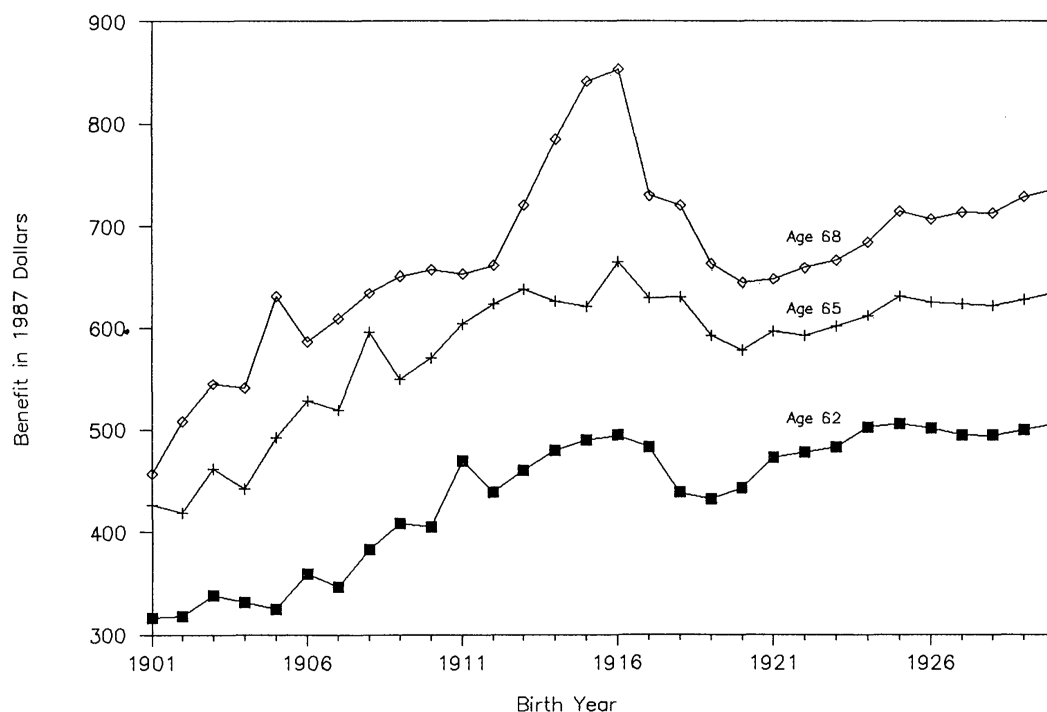


FIG. 1.—Monthly Social Security benefits for selected retirement ages

Figure B.1: Figure is reprinted from Krueger and Pischke (1992), and shows variation in Social Security benefits by birth and retirement years for selected retirement ages. Within a birth cohort those who retire later receive higher benefits. This is substantially amplified by the Notch as shown by individuals who retire at age 68 for whom a retiree born in 1916 receives approximately \$150 more per month than a similar retiree born in 1917 in 1987 dollars.

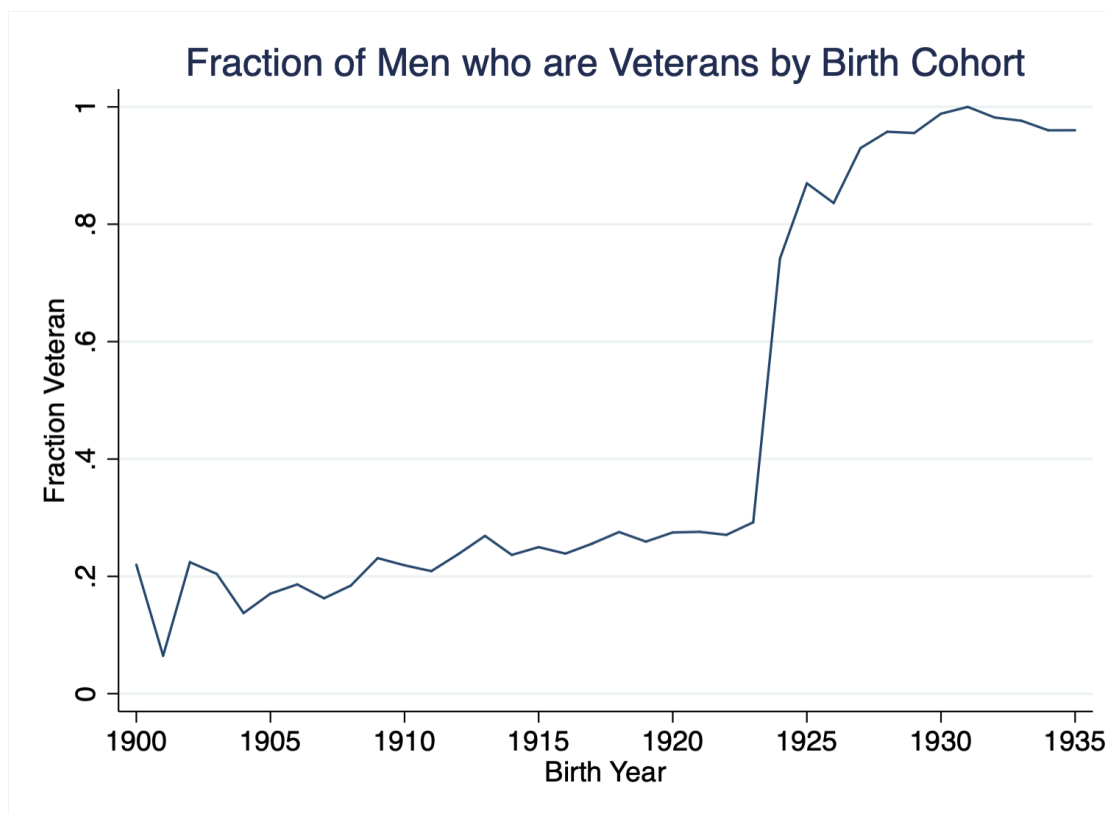


Figure B.2: Author's calculations using the 1993, 1995, 1998, and 2000 AHEAD/HRS surveys. The figure shows the fraction of men in each birth cohort that are veterans.

FIGURE 1: FRACTION OF VETERANS AND COHORT-SPECIFIC AVERAGE RESIDUAL OF REDUCED-FORM MODELS FOR WORK DISABILITY, MEN BORN 1915-1939

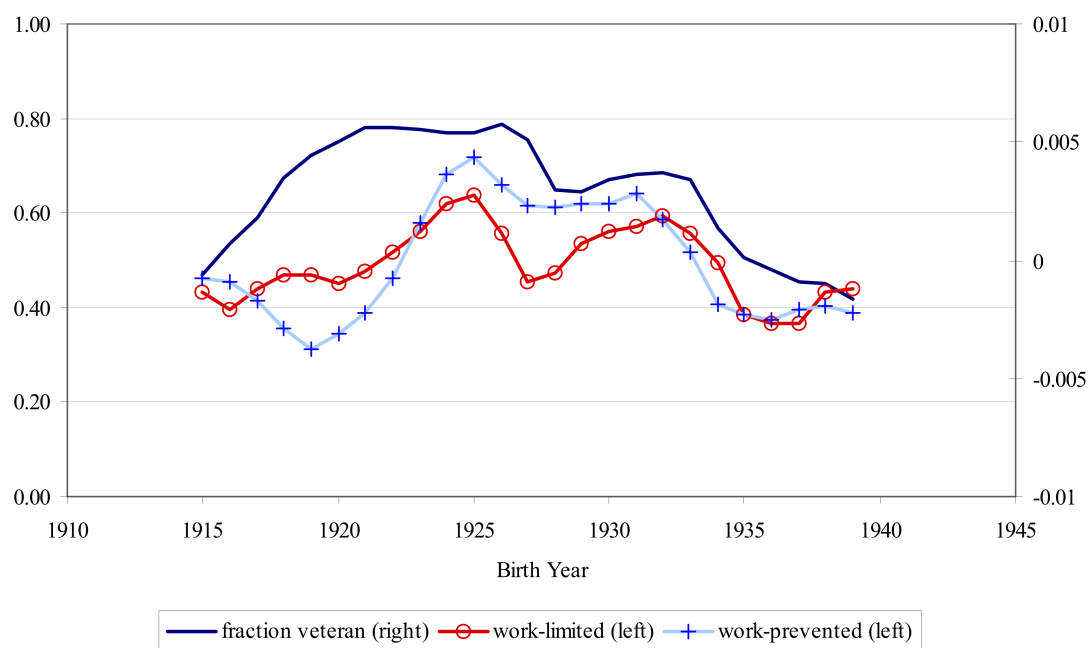


Figure B.3: Figure is reprinted from Bedard and Deschenes (2003). The solid blue line shows the fraction of veterans from each birth cohort. from the 1970-1990 Census.

APPENDIX C
CHAPTER 3 APPENDIX

Below we discuss the construction of our decennial Census and CPS series, and each of the components of our various income measures that we have imputed in some way.

C.1 Comparing Decennial Census-based and CPS-based Income Measures

Because the CPS asks more granular questions regarding sources of income, we must make some imputations to construct a IPMUS Census data series-based equivalent to the four income series we use in the body of this paper: (1) Market Income of Tax Units; (2) Household Size-Adjusted Pre-Tax Post-Transfer of Persons; (3) Household Size-Adjusted Post-Tax Post-Transfer Income of Persons; and (4) Household Size-Adjusted Post-Tax Post-Transfer Income plus Medicare and Medicaid of Persons for decennial Census years 1960, 1970, 1980, and 1990. For instance, it is straightforward to measure (1) with the 1967 CPS since it contains separate questions for: a) Wage and self-employment income, b) business income, c) farm income, d) Social Security, e) Dividends, interest, and rent, f) Welfare or public assistance, g) Unemployment and workmen's compensation, h) Alimony, i) private pensions, and j) anything else. While the 1960 decennial Census contains separate questions for a, b, and c, it lumps all other income into a single "all other income" question. Hence it combines all these other sources of income into a single category.

We add categories (a, b, c, e, h, and i) in the 1967 CPS to estimate market income. This is not possible in the 1960 decennial Census, however, since (e, h, and i) are grouped with (d, f, g, and j). Both the decennial Census and CPS ask increasingly granular questions in later years, but the decennial Census questions are always relatively less granular, and even in the 1990 decennial Census some sources of market income, specifically alimony, are still grouped in the “other income” category. Therefore it is necessary to impute the proportion of “other income” in the Census that should be included in market income for individuals in each decennial Census year.

To do so, we construct a separate definition of income in the CPS that includes all of the income sources in the “other income” category for the decennial Census in the same year (using the 1967 CPS for the 1960 Census). We then follow a procedure similar to that in Burkhauser et al. (2017), specifically their HBAI-SPI2 adjustment. We start by ordering individuals in the CPS for the relevant year by earned income (the sum of wage, farm, and self-employment and business income). We then assign them to percentiles, and within each percentile in the CPS calculate the fraction of the “other income” category that comes from the CPS categories that should count as market income. We match these percentiles to the individuals in the equivalent percentiles in the Census, and use the estimated fractions to assign individuals in the Census an appropriate amount of their “other income” when estimating market income.

This imputation performs well for the higher income quintiles, but less so in lower-income quintiles, and especially the lowest quintile. This problem results from substantial heterogeneity in the composition of low-income tax units and households relative to those in higher income quintiles. Specifically, low-

income units have large shares of those who have low market income because they are relatively young, and those who have low market income because they are past retirement age. However, these groups have rather different amounts and sources of other income, and so estimates assigning the same fraction of other income to both groups perform less well when we compare our decennial Census series to the CPS.

To address this issue, we take the imputation one step further. In addition to estimating the fraction of other income by earned-income percentiles, we split this imputation into three separate groups: young (<20 years), middle (>19 and <65 years), and old (>65 years). In doing so, the relevant fraction of other income is estimated from a population that has more consistently similar sources of other income. While the decennial Census estimates of mean incomes for the first quintile are still less accurate than those for higher income quintiles, the estimates are significantly more accurate than those estimated using only earned income percentiles.

To evaluate our efforts to recreate our CPS income series using the decennial Census, we compare the overlapping years of the two data sets (1969, 1979, and 1989) in Appendix Table C.1. We use five different income measures to compare the series: (1) size-adjusted earned income of tax units, (2) size-adjusted market income of tax units, (3) size-adjusted market income of households, (4) size-adjusted pre-tax post-transfer income of households, and (5) size-adjusted post-tax post-transfer income of households. We compare both the median and mean of each measure for each year.

While there are necessarily random differences due to sampling, some of the measures can be expected to be more similar than others. In particular, we

would expect measure (4): size-adjusted pre-tax post-transfer income of households to be the most similar across data sets, as it is the only series for which no imputations are necessary. Both the decennial Census and the CPS are household surveys, so there is no estimation of analysis units. Furthermore, all of the questions in both surveys regarding income sources counted in pre-tax post-transfer income are grouped such that no estimation is necessary. Thus, we expect measure (4) to be the closest. Measures (1), (2), and (5) all require imputation of tax units, and measures (2) and (3) require imputing sources of market income in the decennial Census.

Panel A compares the medians of the five measures across the two data sets. For each year the estimates of each data set are shown, followed by the percentage difference between the decennial Census estimate and the CPS. As expected, measure (4) is the most similar of the series overall, with the decennial Census estimates slightly overestimating the CPS estimates, but by a difference of less than 2 percent in all three years. The other measures have larger differences between the CPS and the decennial Census measures, but the differences are smaller than 4 percent, with the exception of measure (5): post-tax post-transfer income in 1969, for which the decennial Census estimate is 5 percent smaller than the CPS estimate.

Panel B compares the means of each measure. While the deviations are slightly larger, size-adjusted pre-tax post-transfer income of households is still overall the closest measure, with the largest deviation being a 3.3 percent underestimate of the CPS measure in 1979. Measure (3), the market income of households, now has the largest difference of 6.7 percent in 1979, but otherwise the results are largely similar to those of Panel A. Overall, it is encouraging that

our Census series is relatively accurately replicating the measures of our CPS series, thereby supporting our use of the 1960 Census to create an initial point for the series.

C.2 Estimating Taxes

Similar to Armour et al. (2014), we impute tax liabilities using NBER Taxsim 9.3 based on the year and state of residence for each tax unit within a household. These tax units are assigned using the procedure from Armour et al. (2014), as described in Burkhauser et al. (2012b) and Piketty and Saez (2003). Married couples, divorced or widowed individuals, and single individuals over the age of 20 are all considered their own tax unit, as are never-married children under 20 who live alone.

This process is straightforward for 1979-2007, the years considered by Armour et al. (2014), but poses additional problems for the earlier years of the series. Specifically, NBER Taxsim does not estimate state taxes prior to 1977. Additionally, earlier years of CPS data lack the level of detail for demographic data on households that is contained in later years. As a result, additional assumptions on tax units must be imposed for earlier years. Specifically, prior to 1976 not all states are uniquely identified in the CPS, and are instead grouped as several states or larger regions.

To address this, we use a procedure similar to Fox et al. (2015). We first compute each tax unit's state tax liability for the year 1977 for all households surveyed prior to 1977, regardless of the year the tax unit appeared in the CPS. Tax units for which a unique state is not identifiable are assigned to the state in

the region with the median tax rate in 1977. These state tax liabilities are then deflated using the CPI-R-US, and multiplied by the ratio of state tax revenue in the assigned state for the year the tax unit appears in the CPS relative to 1977, after controlling for population growth. Determining tax unit status of filers versus non-filers is also a difficulty. We follow Burkhauser et al. (2012b) and include all tax units regardless of filing status as it is not possible to differentiate between filers and non-filers prior to 1993. For a discussion of median income analyzing only filers after 1993, see Burkhauser et al. (2012b). As with previous issues, this simple tax model is a first approximation and subject to greater noise than the NBER model used for more recent years.

C.3 Estimating Food Stamps/SNAP

The CPS data contain estimates beginning in 1979 for the market value of the food stamps that families receive. Our imputation procedures for earlier years follow the method in Fox et al. (2015). We describe their method below with particular focus on the few minor differences between our method and their method. We first impute any receipt of food stamps. Conditional on receipt, we then impute the size of the family's food stamp benefit. While the sharing unit in this paper is the household, our imputations are at the family level since that is the administrative sharing unit for food stamps. We then aggregate these benefits up to the household level. To do this, we supplement the CPS data with data from the Consumer Expenditure Survey (CEX), which we downloaded from ICPSR (Bureau of Labor Statistics, 1987). The continuous CEX series does not begin until 1980. However, there were earlier surveys in the years 1960/1961 and 1972/1973. We make use of the 1972/1973 CEX survey, which

contains data on family receipt of food stamps.

To impute receipt, the process is similar to that in Fox et al. (2015). We predict receipt with a logistic model (rather than a linear probability model) using the 1972/1973 CEX to predict family receipt of food stamps based on receipt of public assistance, number of children in the family, unemployment status of the family head, dummies for having one adult or three or more adults in the family, age, education, and race categories for the family head, family size, a dummy for marital status of the family head, and interaction terms for the race and education of the family head. All of these demographic variables are also available in the CPS. Thus, using the values predicted by the logit model in the CEX, we predict the probability of food stamp receipt for family heads in the CPS. We assign food stamp receipt to the household head with the highest predicted probability.

The next issue is to constrain the percentage of families imputed to receive food stamps. We begin by calculating the percentage of family heads receiving food stamps in the 1980 CPS. The USDA published administrative data on caseloads annually back to 1969 (USDA, 2014). We use Statistical Abstracts of the United States to extend this series of caseloads back to 1967 (U.S. Census Bureau, 1973). We then use this series of caseloads for the entire period 1967-1978 together with yearly population estimates (using data from the U.S. Census Bureau, 2000) to calculate the growth in caseloads per capita between years. Using the percentage of families on food stamps in 1980 and projecting back provides us a percentage of the population participating in food stamps for every year. The final imputation of receipt then results from simply constraining the percentage of recipients to those families with the highest computed probability in

each year.

The final step is to assign values of food stamp benefits to those families we imputed to receive them. We begin with the same “hot deck” procedure described by Fox et al. (2015). We divide the sample into mutually exclusive cells based on receipt of public assistance, poverty status, number of children, and number of adults in the family. Poverty status is not recorded in the CEX data, so we assign it using poverty cutoffs from the U.S. Census Bureau (1972), and compare the assigned cutoff to reported family income.

At this point we deviate from Fox et al. (2015) with respect to the method for creating bins and the corresponding cell sizes. They split the CEX sample into 36 cells, and then divided each cell into deciles based on the value of the family’s food stamp benefits. They also split the CPS data into the same 36 cells, and then randomly assigned ten deciles within each cell. The cells and deciles were then matched between the CPS and CEX, and the families in the CPS were assigned food stamp benefits equal to the value of their corresponding cell in the CEX.

Our concern relates to the number of separate bins for children and adults, in addition to poverty status and welfare receipt, that were used to generate the 36 mutually exclusive cells in Fox et al. (2015). Assuming, for instance, cells for 0, 1, or 2 or more children, and 1, 2, or 3 or more adults yield 36 cells. However, only 1,238 families in the 1972/1973 CEX reported receiving any food stamp benefits. Divided evenly this would give an average of under 35 observations per cell, and only 3.5 observations per decile. However, as the 1,238 observations are not uniformly distributed across cells, many cells have far fewer observations, in several cases fewer than 10. This prevents the calculation of food stamp benefit

values for deciles within some cells. Other choices for bin size yield similar difficulties.

To resolve this issue, we select only 16 cells using bins for 0 or 1 or more children in the family, and either 1 or 2 or more adults in the family. This yields somewhat more satisfactory cell sizes, with the smallest cell containing sufficient observations to compute deciles of food stamp benefits, albeit still with a relatively small number of observations.

We then inflate the monetary value of these predicted food stamp benefits to the relevant year using the CPI-R-US (Fox et al. use the CPI-U). We then further adjust the benefit level using the ratio of the average benefit of the year in question to the average benefit level in 1972/1973 that we compute from the USDA series. As described above we then follow the Fox et al. (2015) procedure to match across cells and deciles between the CPS and CEX.

C.4 Estimating the National School Lunch Program

As with Fox et al. (2015), the procedure for computing receipt and benefit value for school lunches is quite similar to the procedure for food stamps. The 1972/1973 CEX survey does not ask questions regarding school lunch receipt. So like Fox et al. we use the family values for school lunches in the 1980 CPS. The percentage of families receiving benefits each year is similarly predicted and constrained using changes in administrative caseload.

Fox et al. (2015) use administrative data from the USDA compiled by Robert Moffitt and his colleagues that goes back to 1969, and, as before, supplement

this series using Statistical Abstracts of the United States. Since we did not have Moffitt's series, we consulted the original sources and used a combination of data from the USDA and the Statistical Abstracts of the United States. We compute monetary values from the 1980 CPS, and deflate them using the CPI-R-US after controlling for population growth.

C.5 Estimating Housing Benefits

Our procedure for housing benefits is similar to our imputation for food stamps, but it is limited to the subsample of families renting their dwelling. Again, without Moffitt's series we referred to the original data sources mentioned by Fox et al. (2015), in this case historical White House Budget Tables for discretionary programs which extend back to 1962 (OMB 2015). This is a series of total program expenditures rather than a series of rental costs per household since it does not contain data on participation. Fox et al. observe that the trend in expenditures per household is roughly linear from 1977 to 2009. Therefore, they assume this trend backwards to 1967 to estimate the number of households participating in housing subsidies. We do the same using a similar series for 1977 to 1997 from Olsen (2003). As with food stamps and school lunches, we then use this series to constrain households receiving subsidies after imputing probability of receipt using the linear probability model.

We deviate from Fox et al. in our computation of the value of housing assistance by again using the matching procedure discussed above for school lunches using the 1980 CPS. Fox et al. attempt to replicate the Census method of taking the lesser of "the shelter portion of the threshold minus estimated

rental payments” and “the market value of the housing unit minus estimated rental payments” (pg 32). Lacking the relevant data to estimate rental payments or market values back to 1967, Fox et al. assume that people spend 30 percent of household income on rent, and estimate the value of the housing assistance benefit as “the shelter portion of the threshold minus the estimated rental payments” (p. 32). They find this procedure overestimates the value of housing assistance on poverty rates, and use a correction factor of 0.89 of the estimated housing value to correct the series.

C.6 Estimating Medicare

Our primary contribution in this paper is that we provide the first estimates of the value of government- and employer-provided health insurance at the household level for 1959-1978. As with other in-kind transfers, this process consists of two steps: first imputing whether or not a household receives health insurance, and second imputing the (ex-ante) value of health insurance, conditional on source and receipt.

Our imputations of Medicare receipt are inexact but relatively straightforward. We first assign Medicare receipt to all individuals over the age of 65 since Medicare was almost universal at this age even in the early years of the program. Medicare eligibility was extended to nearly 2 million additional individuals in 1972 that were under age 65 but had been receiving Social Security Disability Insurance (DI) for at least two years. The CPS data report only receipt of Social Security benefits and do not distinguish between Old-Age Insurance (OAI), Survivors Insurance (SI), and DI and do not provide information on how

long benefits have been received. Since OAI cannot be received before age 62, we assume that those under the age of 62 who report receiving Social Security benefits for the years 1972-1978 are also receiving Medicare. Because we cannot distinguish those receiving OAI and DI who are age 62-64, and because questions related to a person's disability were not asked in the CPS until 1980, we assume no one in this age group is receiving DI. In 1965, the age at which widows could collect SI was reduced to 60 (Achenbaum, 1986). We are able to identify widows and we assume that they are not receiving DI. Our imputation will overstate the number of Medicare recipients to the extent that individuals age 65 or older have not enrolled in the Medicare program primarily because they are still working and have private health insurance and hence do not benefit greatly from Medicare and they are not penalized for not taking it. We will understate Medicare participation for those aged 62-64 whose Social Security benefits are from DI and for widows age 60-61 who are also receiving DI.

C.7 Estimating Medicaid

Our imputations of Medicaid receipt are also inexact but relatively straightforward. Early survey years include a specific income source for "Welfare or public assistance" which does not identify separate programs but can include payments from state old-age assistance programs and aid to the blind or disabled before the implementation of Supplemental Security Income (SSI) in 1974, as well as AFDC receipt. Beginning with the 1976 CPS (for income year 1975), additional income questions added to the CPS allow for more granular identification of family resources. The survey continues the question regarding public assistance, but additionally includes income from SSI as a separate question.

Title XIX of the Social Security Act created Medicaid in 1965. Medicaid expansions were optional and run at the state level, but states were incentivized by matching federal funds to create Medicaid programs. Expanding states were required to cover children in low-income families with single parents on welfare, and also to those receiving aid from state programs for the elderly, blind, and disabled (many states had programs to cover these individuals prior to the federal create of SSI in 1974). As states choosing to expand were required to cover both of these groups, enrollment in AFDC or SSI automatically made individuals eligible for Medicaid in states that expanded (CMS 2005, Rowland 2005).

These are exactly the income categories covered by the CPS question regarding public assistance, and so it may reasonably be inferred that individuals reporting receipt of these sources of income are automatically eligible for Medicaid. Thus we impute Medicaid receipt to any individual reporting income from either source. In addition, we assign receipt to children in a family with a parent reporting income from these sources.

However, Medicaid eligibility and enrollment are not necessarily the same. This is particularly so in later years, after Medicaid eligibility was largely decoupled from enrollment in AFDC or SSI. However, this should pose less of a problem in the early years of the Medicaid program, as eligibility is closely tied to these programs in its early years, and there are few alternative ways to qualify based on federal regulations. Thus we treat eligibility as enrollment for the years 1967-1978. States did have the option to independently expand coverage further, but received no matching federal funds and so we do not consider such extensions.

States choosing to participate in Medicaid were required to provide coverage to families receiving any of these sources of public assistance. Therefore, prior to 1974 we can assume anyone reporting income from these pre-SSI public assistance programs was also eligible for Medicaid. When we combine this with basic information on family structure (i.e., ages and number of children) we can then assign actual receipt.

C.8 Estimating Employer-Provided Health Insurance

Imputing coverage of private health insurance is more difficult than it is for Medicare and Medicaid because private coverage is neither universal nor based on uniform eligibility criteria, and no questions were asked about it on the CPS prior to 1980. Additionally, it is known that Medicaid coverage “crowds out” employer-provided health insurance (Cutler and Gruber, 1996). Nonetheless, our basic imputation strategy is similar to that we have discussed above for food stamps and housing subsidies. We use the 1980 CPS with a linear probability model to predict employer-provided health insurance coverage for employed individuals in the CPS, and extend coverage to their immediate family members as well.

As with housing subsidies, the lack of administrative data provides a challenge, both to constrain the percentage of individuals covered by employer-provided health insurance and to determine the market value of the benefits. The NHEA data provide estimates of the yearly total expenditures on private health insurance, but there is a lack of data on number of covered individuals. To resolve this, we use the average market value of benefits in the 1980 CPS and

project it back using the CPI for medical care. In doing so, we will underestimate the value of employer-provided health insurance to income growth, as this effectively holds the real value of such coverage constant. It is likely that the market value of employer-provided health insurance grew in real terms over the period 1967-1978, as there is significant growth in other income sources over this period.

Dividing yearly total expenditures from the NHEA by our estimates of the average market value of coverage from private health insurance provides an estimate of the number of recipients of employer-provided health insurance. As with other in-kind benefits, we combine this estimate with U.S. population data from the Census to obtain a yearly estimate of the proportion of the population receiving employer-provided health insurance. This estimate is then used to constrain our receipt imputation using the predicted values from the linear probability model, and recipients are assigned the estimated market value of employer-provided health insurance.

Our method for estimating the market value of employer-provided health insurance is more inexact than our method for estimating Medicare or Medicaid. In addition to understating the real growth in market value, it also does not account for crowd-out of private insurance by Medicaid. The CPS does not contain any information to address these issues.¹

We do not extend our analysis of the market value of employer-provided health insurance to 1959. Unlike the case for Medicare and Medicaid in which their value was zero in 1959, we suspect that the value of employer-provided

¹It may be possible to use information from the 1977 National Medical Expenditure Survey or the 1960/1961 and 1972/1973 CEX, all of which contain information on the receipt of private health insurance benefits, to supplement the information we are using here.

health insurance was non-trivial. But we have not been able to find a plausible way to capture that value in the aggregate and assign it to our 1959 population in the decennial Census.

C.9 Imputation Results

Medicare Imputations. Appendix Figure C.1 shows the proportion of individuals we impute to receive Medicare. Here “Official Participation” is the total number of enrollees based on aggregate administrative data divided by the total U.S. population. “CPS Participation” is the percentage of individuals reporting participation in the CPS. The participating proportion in the CPS is expected to be somewhat lower than that of the general population since the CPS samples the non-institutionalized population of the U.S., whereas some Medicare enrollees are institutionalized. The “Over 65” series shows the proportion we impute to be participating, using only those who are over the age of 65. The “Over 65 plus Disability” series includes those who report receiving Social Security and are under the age of 62.

Our preferred series, which includes those reporting Social Security benefits under the age of 62 for the first time, tracks the 1972 increase in aggregate per capita Medicare eligibility in that year when an additional 2 million individuals on the DI program increased total enrollment by around 10 percent. Our imputation successfully captures this expansion of Medicare receipt, although the increase in enrollment of 1.6 percentage points slightly overstates the size of the increase. This may be in part due to Medicare enrollment requiring 24 continuous months of SSDI receipt to receive coverage. However, as we only

observe Social Security receipt in one year, those who are receiving Social Security through DI but have been receiving it for less than 24 months will be imputed to receive Medicare even though they are not yet eligible.

The second historical event observed in Appendix Figure C.1 is that the CPS first began including questions relating to health insurance in 1979. While we use this series in income calculations from 1979 to present, we extend our imputations of receipt to these years for the sake of comparison. It appears that, if anything, the accuracy of our imputation improves in more recent years, at least relative to receipt as measured in the CPS.²

Medicaid Imputations. Appendix Figure C.2 shows the proportion of individuals we impute to receive Medicaid. Here “Official Participation” is the total number of enrollees based on aggregate administrative data from each state divided by the total U.S. population.³ “CPS Participation” is the participation rate as reported through the CPS, which as with Medicare begins in 1979, the first year the CPS asks questions regarding health insurance. Our “Imputed Participation” series is based on CPS questions between 1967 and 1979 regarding the receipt of public assistance. While its level is lower than the one based on aggregate administrative data its trend is similar. It is known that Medicaid receipt is

²In future work we will try to improve our imputation for the early period of the series by identifying sub-groups of the under-62 Social Security recipients that would not be eligible for Medicaid. For instance, widows are able to receive Social Security benefits for the deceased spouse, and in 1965 the age at which widows were eligible to receive this benefit was reduced to 60 (Achenbaum, 1986). Thus widows age 60-61 receiving Social Security may be receiving it through SSDI and thus are eligible for Medicare, or may receive Social Security as a survivor and thus are not eligible for Medicare until they reach 65. We cannot distinguish between the two cases, and thus drop widows age 60-61 but retain widows under 60, who cannot receive Survivor’s benefits and thus are likely reporting SSDI income.

³As Medicaid is a state-level program, aggregate administrative data is less readily available than the federally run Medicare program. This series was collected as part of the actual imputation process, and ongoing work will collect the relevant data for future years for the purpose of comparison to our imputation and the CPS series. We cannot compare to CPS participation rates during this early period as the CPS has no data on participation.

underreported in the CPS (Davern et al. 2009), and thus it is not surprising that the CPS and official series do not quite match.

Although the imputed series approximately follows the trend of the official series, there are some inconsistencies, particularly in the early years. Some of this inconsistency is to be expected mechanically due to data constraints for state of residence combined with states implementing Medicaid programs in different years. Early years of the CPS do not uniquely identify all states, and in many cases states are grouped together with the same code. For example, for the 1968-1971 surveys, 30 codes are used to identify the 50 states and the District of Columbia. Only 26 states had implemented Medicaid programs as of January 1967. Eleven more implemented programs at some point during 1967, two in 1968, three in 1969, and seven during 1970. The final two, Alaska and Arizona, implemented their programs in 1972 and 1982 respectively (Advisory Commission on Intergovernmental Relations, 1968; Gruber, 2003).

Thus, many states implemented their programs during our sample period, but in our imputation will be assigned receipt in the wrong year depending on the other states using the same state code. If multiple states with the same code created programs in different years, we assign receipt based on the year of the most populous state(s) in the group. Therefore our trend must mechanically differ from the official trend, as some families will erroneously be imputed to be covered by Medicaid when their state had not yet expanded, or not be imputed when their state had already expanded.

For example, North and South Carolina share the state code 57, but North Carolina expanded in January 1970 whereas South Carolina expanded in July 1968. As North Carolina has a larger population, all families with this state code

are assigned no Medicaid coverage until 1970, even though many would be covered if South Carolina could be identified. A second example: North Dakota, Nebraska, Kansas, and South Dakota all receive the state code 49. However, North Dakota and Nebraska both implemented Medicaid programs in 1966, and thus individuals are covered throughout our entire sample. Kansas and South Dakota both expanded in mid- or late-1967, and thus families should not be imputed to receive Medicaid until 1968. As South Dakota and Kansas have a larger population in 1967 than North Dakota and Nebraska, all families from these states are coded as not receiving Medicaid in 1967. Overall, nine state groupings for the creation of Medicaid have one or more states with incorrect imputations for Medicaid receipt, for a total of 12 miscoded states.⁴

After 1974 the imputed series appears to match the trend more accurately—although not the level—and it maintains the same trend as the official and CPS participation series until the late 1980s. This is not surprising; because of the substantial changes made to Medicaid eligibility in the late 1980s, our imputation method should not be expected to maintain the same trend after this period. In 1986 Medicaid was expanded to allow states to (optionally) cover pregnant women and infants living in families with income up to 100 percent of the FPL. In 1988 the coverage to pregnant woman and infants in families with income up to 100 percent of the FPL was converted to a mandatory part of state Medicaid programs, in addition to coverage changes for individuals with institutionalized spouses and the implementation of the qualified Medicare Beneficiary program under which low-income elderly could get Medicaid assistance in paying for their Medicare premiums. In 1989, Medicaid coverage was expanded to pregnant women and children under age 6 with family income less than 133 percent

⁴The miscoded states are New Hampshire, North Dakota, Nebraska, Iowa, South Carolina, Delaware, Arkansas, Arizona, New Mexico, Idaho, Utah, and Alaska.

of the FPL (CMS, 2005).

These changes all increased the options for low-income families to obtain Medicaid coverage independent of receipt of AFDC or SSI. Thus it is reassuring that imputed receipt no longer appears to follow the same trend as receipt as measured in the CPS, as none of the changes are accounted for in our imputation. Finally, in 1996 Welfare was reformed and AFDC was replaced with Temporary Assistance for Needy Families (TANF) (CMS, 2005). The link between Medicaid and Welfare was completely severed, and thus our imputed series diverges even further from the CPS series.

As mentioned above, it is known that Medicaid receipt is under-reported in the CPS. However, our imputation reports roughly 1 percent lower population coverage during the period for which it overlaps with the CPS measure and is expected to be similar (1979-1986). There are several reasons our imputation may under-measure actual receipt. In early years, states with Medicaid coverage may be miscoded due to state groups. Additionally, there are several different groups eligible for Medicaid, and it is likely that our imputation strategy does not capture all of them. States with Medicaid programs are required to provide coverage to the recipients of any public assistance program (the categorically needy), to anyone who would be eligible for public assistance but does not meet a state-imposed eligibility requirement (categorically related needy), anyone under 21 who would be eligible for AFDC except for a state age or school-attendance requirement (also categorically related needy) (Advisory Commission on Intergovernmental Relations, 1968). These latter two categories cannot be identified by our simple imputation using receipt of public assistance, and would require a much more arduous procedure of coding up state rules re-

lated directly to these two groups.

States can choose to cover some additional populations that receive federal funding contributions. These include individuals who meet federal requirements for public assistance programs but not the state requirements if their state of residence has stricter rules; individuals who are ineligible for public assistance due to being a patient in a medical facility; individuals who are aged, disabled, blind, or in families with dependent children that have sufficient income to cover living expenses but not cannot afford their medical care; and, finally, anyone who is medically needy and under the age of 21 but not eligible for assistance from other federal public assistance programs (Advisory Commission on Intergovernmental Relations, 1968). To the extent that these populations exist, our imputation measure will underestimate Medicaid recipients. As a result of our under-imputation of receipt, our results for Medicaid's effect on household income are likely lower bounds for the importance of Medicaid.

Employer-Provided Health Insurance Imputations. Appendix Figure C.3 shows the proportion of individuals we impute to receive employer-provided health insurance. There is no "official" series due to a lack of data regarding employer-provided health insurance during this time period. The series for CPS receipt indicates a roughly 10 percent drop in the population receiving employer-provided health insurance coverage during the period from 1979 to 2013. In this case, the imputation for private insurance coverage is significantly more difficult than for Medicare or Medicaid, as there is a lack of defined rules for receipt. Therefore, our preliminary strategy adopts the method used for other in-kind transfers.

The NHEA contain a series for expenditures on "Private Health Insurance."

There are two primary issues with the use of this series for our purposes. Firstly, private health insurance and employer-provided health insurance are not synonymous, and private health insurance may include things such as plans purchased directly on the market. Secondly, this series does not distinguish between employer contributions to health insurance plans, and the premium contributions of employees. The imputation in Appendix Figure C.3 ignores both of these issues, and assumes all the spending on private health insurance reported in the NHEA is from employers on employer-provided health insurance. Both of these differences will lead to overestimates of the value of employer-provided health insurance.

Similar to our estimates of housing subsidies, our data suffers from a lack of information on participation, both in administrative data and in the CPS. Participation data is necessary to constrain the number of individuals imputed to receive employer-provided health insurance. The 1979 CPS contains data on the value of employer contributions to health insurance premiums in that year. We begin by projecting the average value of employer contributions back to 1967 using the CPI for medical care. This holds the value of health insurance constant in real terms, and thus almost certainly understates the extent to which growth in employer contributions to health insurance contributed to growth in household income over this period, given that other sources of income grew rapidly, with the exception of the recession in the early 1970s.

To obtain estimates of the percentage of the population receiving employer-provided health insurance, we divide total expenditures on private health insurance measured in the NHEA by our estimates of the yearly average value of such coverage. This will mechanically underestimate the percentage receiv-

ing coverage to the extent that real growth occurred in the average value of employer-provided health insurance, as the denominator (our yearly estimate of the average value of coverage) is too large. Indeed, Appendix Figure 3 shows a dramatic rise in the covered population between 1967-1979. While it is possible private coverage was expanding during this period, this would be somewhat surprising given the creation of Medicaid and its potential for crowding out private coverage. Thus the extent of the rise is almost certainly overstated. However, the fact that the estimates of receipt for 1978 and 1979 are very similar is reassuring that this strategy is not without merit.

The final step is imputing the probability of receipt using a linear probability model and constraining recipients to those with the highest probability of coverage. Receipt is imputed only for individuals who report income from wages, and is assumed to cover all immediate family members.⁵

Appendix Table C.2 shows the administrative expenditures for each of the three primary insurance sources, as well as the expenditures predicted by the CPS series and the ratio of the two. This gives us a measure of how well the predicted series captures the total expenditures on each insurance source and shows the accuracy of the series we extend to the early years as compared to the Census estimates beginning in 1979. Note first that the Census estimated values for 1979 onward do not match the aggregate expenditure number for either Medicare or Medicaid. This is primarily because the CPS excludes institutionalized individuals from its survey population and this population requires far more expensive medical services than does the non-institutionalized popula-

⁵Improving the estimates of the value and receipt of employer-provided health insurance is the primary focus of continuing work on this paper. In particular, we aim to utilize the early CEX surveys and the 1960 Census, both of which have some information on health insurance, although still less than ideal administrative data.

tion.

The Medicare series captures approximately the same proportion of aggregate spending in 1967 as in 1979, the first year for which the estimates are based on values estimated by the Census Bureau. However, the proportion of captured spending rises over time between 1967 and 1978, and in fact closely matches the level of total expenditure in the last few years. However, this means the series is overstating the spending in these years, as our estimates are derived from market values based on total program spending, and not spending on our sample population, which excludes institutionalized individuals. We do not have series of total Medicare or Medicaid spending split between institutionalized and non-institutionalized individuals. Future work can refine these estimates by using market value estimates specific to various eligible groups (for example retirees versus the disabled). Overall, the series seems largely plausible.

The story is reversed for our Medicaid series, which captures nearly all of the total Medicaid spending in the early years, and steadily less in later years. The final year of our estimates, 1978, captures roughly the same proportion of program spending as do the years 1979 and after, for which we have the Census estimates, albeit at a slightly higher proportion in 1978. Overall, this series also seems plausible, although similar refinements as mentioned for Medicare would improve the accuracy of our Medicaid estimates as well. While the rising proportion of Medicare spending captured may lead to an overestimate of income growth due to Medicare, the opposite is true for Medicaid, and so the direction of any bias is unclear.

The series for private health insurance is similar to Medicaid. It captures a

large proportion of total spending in 1967 and declines to approximately 60 percent in 1978, a value slightly less than that the Census captures in 1979. Thus, overall, our series estimating values for the three major sources of health insurance seem largely plausible, although future refinements to the estimates may improve them somewhat.

C.10 Extended Income Measures

Appendix Figure C.4 extends a subset of income measures to include the most recent CPS data for income year 2016. Additionally, this figure includes other measures not included in the body of the paper. These new measures are: the labor earnings of tax units; the household size-adjusted labor earnings of persons; and the household size-adjusted market income of persons. The additional series require imputations for more sources of income. As a result, only the household size-adjusted pre-tax, post transfer income of persons and the household size-adjusted post-tax, post-transfer plus in-kind transfer income of persons series extend for the entire time period of 1959-2016. Future drafts will extend these other series to earlier years as well.

The extension of the series to the most recent CPS years also requires an adjustment of the entire series due to the CPS redesign for the 2014 survey year. Our adjustment is effectively the same as the adjustment used for the 1992-1993 survey break, with the exception that the 2014 redesign included a redesigned and traditional CPS sample in the same year, whereas the earlier redesign has the traditional survey for the entire 1992 sample, and the redesigned survey for the entire 1993 sample. Thus, we use the two samples in the 2014 survey to

calculate an adjustment factor such that the measures are the same between the traditional and redesigned surveys, and apply this factor to the income measures in all previous years. While the more recent numbers should be comparable with the estimates from earlier survey years, we indicate this “break” in our income measure with a dashed vertical line, as we did with other breaks in the surveys used.

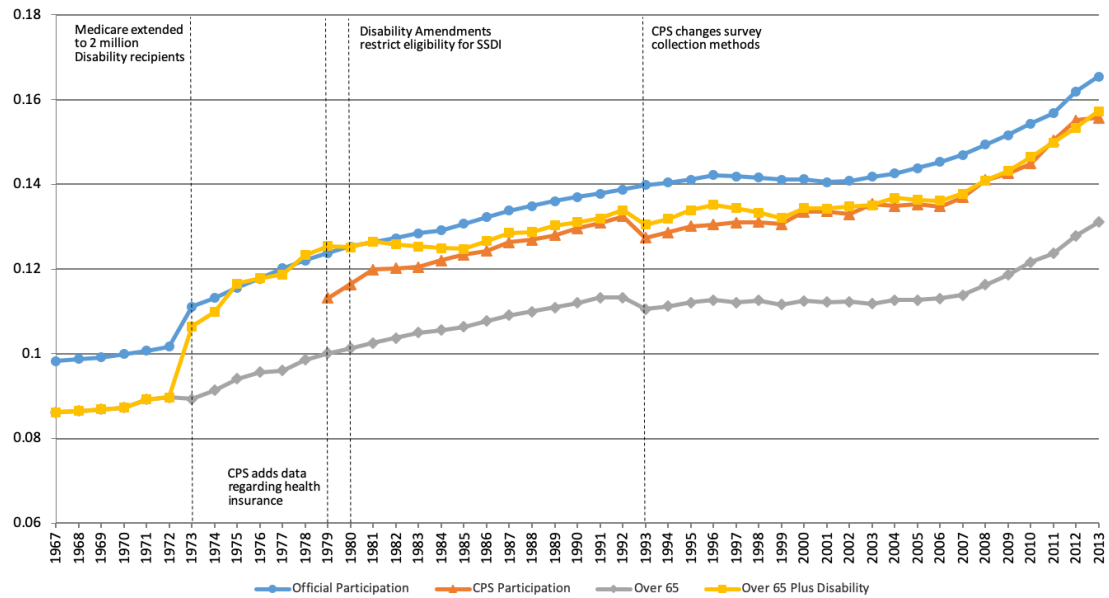


Figure C.1: Measures of Imputed Medicare Eligibility: 1967-2013.

Sources: Authors' calculations using the March CPS, Medicare & Medicaid Statistical Supplement (CMS 2001, 2013ab).

Notes: Official participation measured by CMS in Medicare & Medicaid Statistical Supplements. CPS participation measured from 1979 on. Participation is imputed prior to 1979 to those over 65 and under 62 reporting Social Security income (disability recipients), but excluding widows age 60-61 who may instead be receiving survivors' benefits. Imputation extended to later years to compare to CPS and Official series.

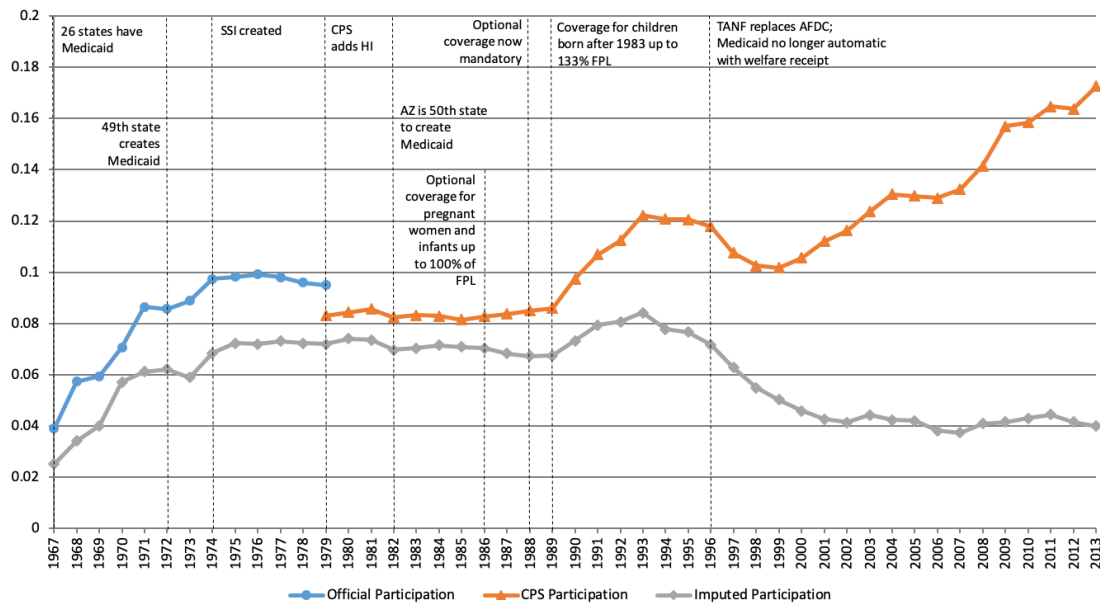


Figure C.2: Measures of Imputed Medicaid Eligibility: 1967-2013.

Sources: Authors' calculations using the March CPS, Institute for Medicaid Management (1979), Klemm (2000)..

Notes: Official participation collected from Health Care Financing Administration (HCFA) records. CPS participation measured from 1979 on. Participation is imputed prior to 1979 to those reporting public assistance or SSI income. This excludes some eligible groups, but includes those who were automatically eligible for Medicaid. 49 states implemented Medicaid by 1972, but not all states are individually identifiable in the CPS, necessarily adding error to early imputation. Imputation extended to later years to compare to CPS and Official series.

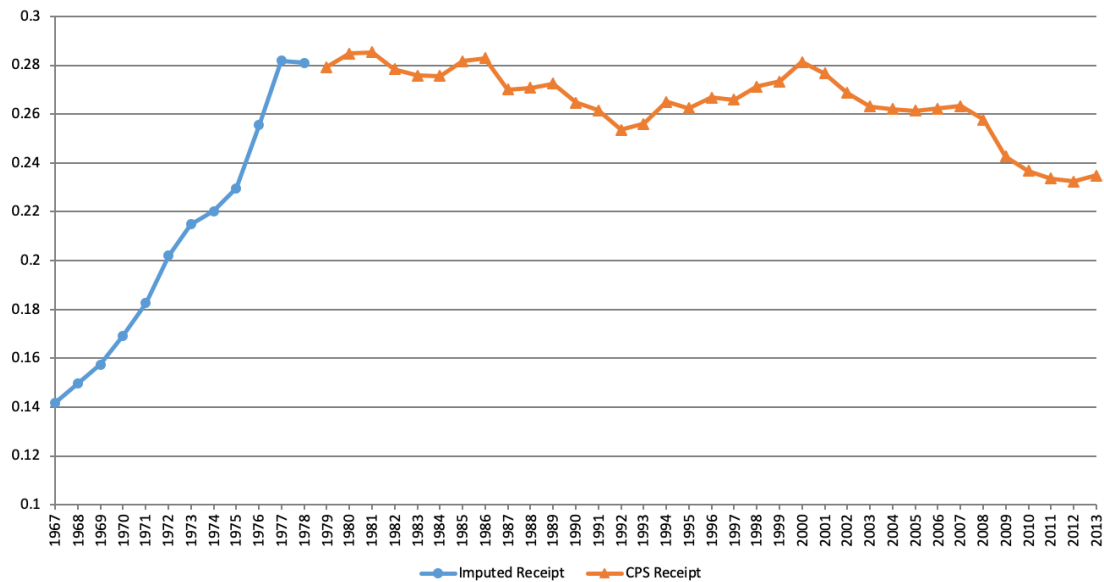


Figure C.3: Imputed Employer-Provided Health Insurance: 1967-2013.

Sources: Authors' calculations using the March CPS and NHEA (CMS 2013b).

Notes: CPS participation measured from 1979 on. Pre-1979 participation imputed using linear probability model based on 1979 CPS following method used by Fox et al. (2015) for other in-kind benefits. Recipient population constrained by dividing total expenditures from NHEA by estimated individual market value. Individual market value estimated by deflating average 1979 benefit using CPI for medical care, thus likely understating percentage of population receiving employer-provided health insurance.

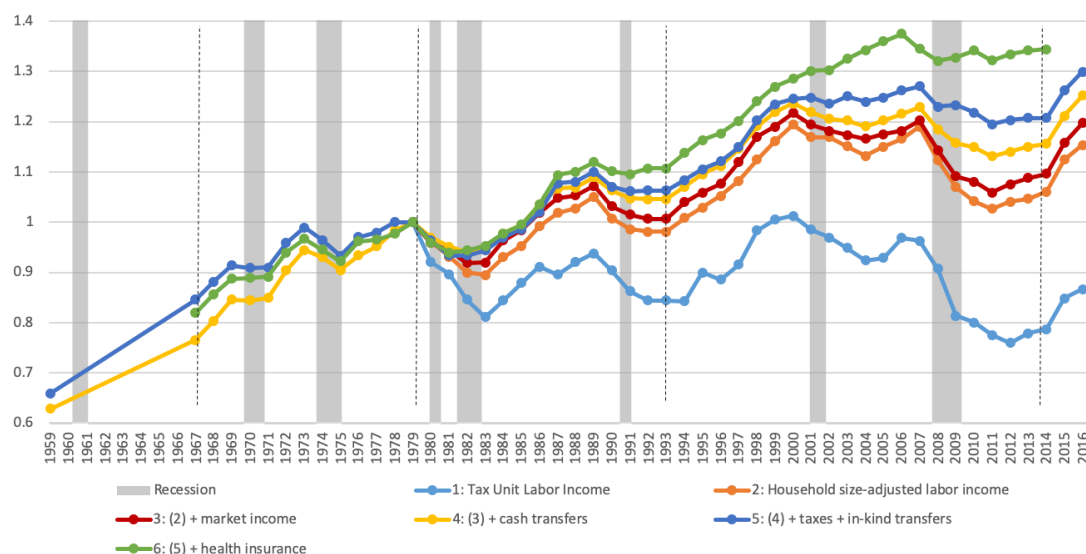


Figure C.4: Extended Median Income Measures Including Recent CPS Data, Normalized to 1979 Levels (1959-2016). *Sources:* Authors calculations using the March CPS, NHEA, White House Budget Historical Tables, Statistical Abstracts of the U.S., Census Bureau population estimates and poverty thresholds, USDA Child Nutrition Tables, and 1972-1973 CEX. Taxes calculated using NBER TaxSim. *Notes:* Median income trends normalized to one in 1979 with NBER recession dates in gray. Series 1 includes the size-unadjusted labor income of tax units. Series 2-6 are household size-adjusted income of persons measures using the square-root of household size, and sequentially adding additional sources of income. Series 6 includes the market values of Medicare and Medicaid, as well as employer contributions to health insurance premiums. As we do not have estimates for the value of employer-provided health insurance in 1959, we begin the last measure in 1967. Changes between surveys or major survey redesigns are indicated by dashed vertical lines.

Table C.1: Comparisons of CPS-based and Decennial Census-based Medians and Means (1969, 1979 and 1989)

	1969			1979			1989		
	CPS	Decennial Census	%	CPS	Decennial Census	%	CPS	Decennial Census	%
Panel A:									
Medians									
Tax Units									
Labor income	25937.62	24568.97	5.6%	22436.91	22234.48	0.9%	21188.7	21574.48	1.8%
Market income	30178.51	29224.14	3.3%	27045.03	27810.31	-2.8%	29424.53	28553.25	3.1%
Size-adjusted households									
Market income	23360.49	23553.52	-0.8	27124.34	26502.40	2.3	29168.89	29877.47	-2.4
Pre-tax post-transfer	24272.95	24635.30	-1.5	28699.23	29036.46	-1.2	31328.28	31887.29	-1.8
Post-tax post-transfer	21783.50	20747.55	5.0	23926.02	24234.00	-1.3	26320.04	26978.65	-2.4
Panel B:									
Means									
Tax Units									
Labor income	31057.38	31055.98	0.0%	33762.85	32388.03	4.2%	33762.85	34801.38	-3.0%
Market income	37711.18	36617.87	3.0%	39432.91	38723.72	1.8%	39022.75	38182.52	2.2%
Size-adjusted households									
Market income	27700.57	27263.15	1.6	32489.49	30451.8	6.7	37205.75	36552.76	1.8
Pre-tax post-transfer	29307.34	28917.7	1.3	34922.41	33802.4	3.3	40138.7	39120.89	2.6
Post-tax post-transfer	24794.20	23581.7	5.1	26762.95	27318.11	-2.0	31171.02	31888.24	-2.2

Sources: Authors' calculations using the March CPS and the decennial Census. Taxes calculated using NBER TaxSim.

Notes: Percentage differences are the deviation of the decennial Census from the CPS.

Table C.2: Official and CPS Health Insurance Expenditures (in Millions).

Year	NHEA Medicaid	CPS Medicaid	Medicaid Ratio	NHEA Medicare	CPS Medicare	Medicare Ratio	NHEA Private Insurance	CPS Employer- Provided	Private Insurance Ratio
1967	3,141	2,574	0.8194	4,924	3,741	0.7597	10,382	11,321	1.0904
1968	3,541	3,241	0.9152	6,218	4,792	0.7706	11,754	12,243	1.0416
1969	4,174	4,350	1.0423	7,045	5,531	0.7851	13,287	13,337	1.0038
1970	5,290	5,020	0.9490	7,672	6,195	0.8075	15,424	14,491	0.9395
1971	6,695	5,713	0.8533	8,443	7,048	0.8347	17,757	15,903	0.8956
1972	8,314	6,715	0.8077	9,325	8,058	0.8641	20,593	16,836	0.8175
1973	9,423	6,645	0.7052	10,730	8,620	0.8033	22,884	16,264	0.7107
1974	11,073	7,920	0.7152	13,428	11,132	0.8290	25,972	18,124	0.6978
1975	13,446	7,879	0.5860	16,336	11,454	0.7012	30,461	17,707	0.5813
1976	15,188	9,685	0.6377	19,694	19,283	0.9791	37,295	25,550	0.6850
1977	17,464	11,317	0.6480	22,891	22,836	0.9976	45,538	28,674	0.6297
1978	19,465	12,554	0.6449	26,668	27,773	1.0414	52,347	31,609	0.6038
1979	22,332	10,368	0.4643	30,922	22,371	0.7235	60,768	54,020	0.8890
1980	26,032	13,618	0.5231	37,387	27,093	0.7247	69,047	51,982	0.7528
1981	30,307	15,465	0.5103	44,769	33,250	0.7427	81,608	59,294	0.7266
1982	32,011	16,066	0.5019	52,351	39,591	0.7563	94,012	69,179	0.7359
1983	35,266	17,985	0.5100	59,559	40,815	0.6853	104,893	76,311	0.7275
1984	38,233	20,693	0.5412	66,207	44,501	0.6721	118,884	79,640	0.6699
1985	40,937	19,113	0.4669	71,829	50,157	0.6983	131,190	87,208	0.6647
1986	45,383	21,129	0.4656	76,829	64,962	0.8455	136,025	91,256	0.6709
1987	50,339	22,758	0.4521	83,081	68,861	0.8288	149,210	106,982	0.7170
1988	55,080	23,723	0.4307	88,965	74,503	0.8374	175,906	118,486	0.6736
1989	61,952	28,209	0.4553	101,137	82,348	0.8142	204,997	128,005	0.6244
1990	73,661	36,317	0.4660	110,182	90,787	0.8184	234,230	136,591	0.5831
1991	93,211	44,426	0.4766	120,617	99,226	0.8227	255,603	140,357	0.5491
1992	108,186	52,676	0.4869	135,996	113,371	0.8336	275,557	162,245	0.5888

1993	122,373	62,196	0.5082	149,965	120,178	0.8014	296,322	186,291	0.6287
1994	134,414	63,709	0.4740	167,670	134,441	0.8018	309,592	207,453	0.6701
1995	144,862	68,690	0.4742	184,393	149,737	0.8121	326,916	214,437	0.6559
1996	152,170	76,582	0.5033	198,750	165,022	0.8303	346,032	218,008	0.6300
1997	160,849	68,265	0.4244	210,376	179,893	0.8551	362,796	205,491	0.5664
1998	169,029	66,383	0.3927	209,420	183,449	0.8760	388,274	208,186	0.5362
1999	183,534	64,588	0.3519	213,173	180,310	0.8458	419,979	211,790	0.5043
2000	200,483	71,597	0.3571	224,829	190,110	0.8456	459,839	228,785	0.4975
2001	224,236	115,734	0.5161	247,686	211,121	0.8524	503,297	250,447	0.4976
2002	248,218	123,178	0.4963	265,381	223,755	0.8431	561,450	278,845	0.4967
2003	269,105	132,731	0.4932	282,699	237,563	0.8403	615,749	316,828	0.5145
2004	290,917	166,115	0.5710	311,158	290,446	0.9334	659,981	371,961	0.5636
2005	309,538	190,222	0.6145	339,800	309,607	0.9111	703,213	397,604	0.5654
2006	306,865	202,219	0.6590	403,731	331,033	0.8199	740,200	410,825	0.5550
2007	326,057	173,744	0.5329	432,837	255,748	0.5909	777,689	397,198	0.5107
2008	344,676	169,783	0.4926	467,066	331,916	0.7106	808,027	408,083	0.5050
2009	374,946	197,149	0.5258	499,723	314,580	0.6295	833,076	407,650	0.4893
2010	397,650	208,586	0.5245	519,899	422,710	0.8131	862,214	405,338	0.4701
2011	407,475	252,027	0.6185	544,678	428,721	0.7871	899,359	420,658	0.4677
2012	423,698	242,471	0.5723	566,629	486,027	0.8578	935,685	426,493	0.4558
2013	449,389	261,265	0.5814	585,701	463,131	0.7907	961,741	431,685	0.4489

Sources: Authors' calculations using March CPS and NHEA.

Notes: NHEA columns give spending on Medicare, Medicaid, and private health insurance. As noted in text, private health insurance will differ from spending on employer-provided health insurance to the extent that individuals purchase plans unrelated to employment, and to the extent that individuals contribute to premiums for plans through employers. CPS columns give the aggregate expenditures on each program as measured in the CPS using CPS-estimated market values post-1979, and our estimated market values pre-1979. Ratio columns give the ratio of CPS expenditures divided by NHEA expenditures.

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